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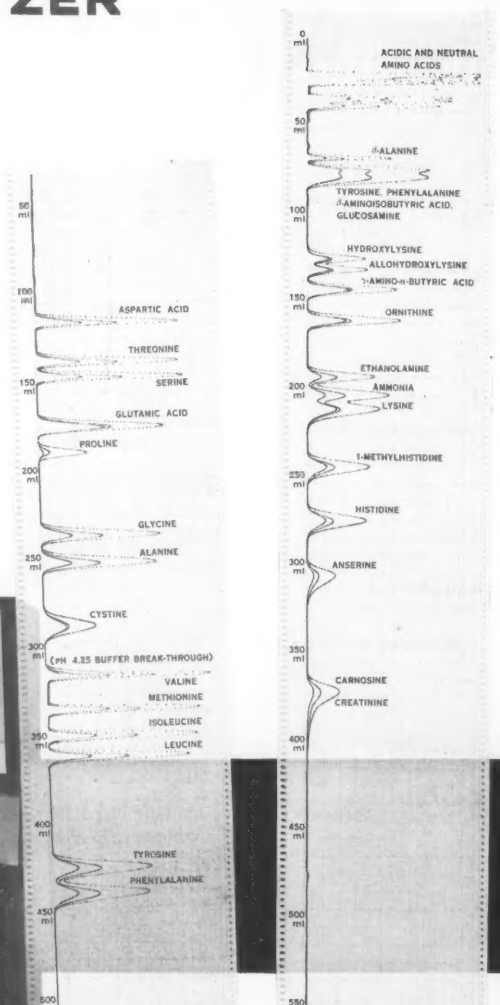

Reference: *D. H. Spackman, W. H. Stein, and S. Moore, "Automatic Recording Apparatus for use in the Chromatography of Amino Acids", Anal. Chem., 30, 1190-1206, 1958.*

Chart at left is analysis of amino acids found in protein hydrolyzates: top run on 150-cm column at 50°C and pH 3.25 buffer, with changeover to pH 4.25 buffer at 6 hours; lower run on 15-cm column at 50°C and pH 5.28 buffer

Chart at right shows components in physiological fluids: 50-cm column at 30°C and pH 4.26 buffer, with changeover to 50°C at 14 hours.

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More Talent for the Asking

A recent publication of the U.S. Office of Education reports the estimate that one-third of the top quarter of the nation's high-school graduates fail to go to college because they lack funds. But no one has firm statistics on which people do not go to college or on the factors that may dissuade or prevent them from going. To get better information on such matters, a two-day battery of tests will be given during the coming school year to about half a million students in approximately 14,000 secondary schools throughout the country. The program is supported primarily by the U.S. Office of Education, with other government agencies assisting, and is administered jointly by the University of Pittsburgh and the American Institute for Research.

One of the program's objectives is to take an accurate inventory of the talents of the nation's secondary-school students. A second, and longer range, objective is to correlate test scores of individuals with their subsequent histories, and thus to provide information for use in school counseling. A third objective is to evaluate the effects of such educational practices as accelerated programs for gifted students. There have been earlier studies along these lines, but nothing approaching the present scale has been attempted before.

The sample of half a million, which is 5 percent of the present secondary-school population, is large enough to insure that a significant number of cases will fall in various small but important categories. In the study of aptitudes and motivation of students, for example, it might be interesting to have such categories as atomic physicists from underprivileged neighborhoods. And, in the study of the effects of different educational practices, it might be interesting to have such categories as women engineers from small schools.

The tests will seek to assess not only a student's aptitudes but also his interests, personality, and achievements, especially in reading and mathematics. The tests will parallel many of those now employed and judged successful, but will be prepared for use only in this program. In addition, a student will be asked questions about his aspirations, family and community background, and health. Also planned are follow-up studies at intervals up to 20 years from the time of the test. Although contact with some students may be lost, it should be possible to trace back important groups. Thus, if the sample is well selected, then 20 years from now about 5 percent of, say, the fellows of the American Physical Society of the proper age range will have taken the test, and they can be matched with their test records by their names and other information.

Studying people in the social sciences differs from studying things in the natural sciences, for people may resent what they interpret as another person poking his nose in their business. The subjects to be investigated may refuse to cooperate with the investigator. Last June in Houston, Texas, for example, 5000 answer sheets in a student testing program were destroyed by the Houston School Board because some parents objected to certain questions designed to measure attitudes and background. Among the questions objected to were a few that were being tried out for use in the coming nationwide effort. The possible contribution of the test program to the more effective use of the nation's talents is great, but success will require circumspection in choosing the questions as well as a cooperative attitude on the part of the students and their families.—J.T.

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Explanation and Prediction in Evolutionary Theory

Satisfactory explanation of the past is possible even when prediction of the future is impossible.

Michael Scriven

The most important lesson to be learned from evolutionary theory today is a negative one: the theory shows us what scientific explanations need not do. In particular it shows us that one cannot regard explanations as unsatisfactory when they do not contain laws, or when they are not such as to enable the event in question to have been predicted. This conclusion, which is contrary to the usual view of scientific explanation (*1*; *2*, pp. 319–352), has important consequences for research in those subjects in which serious errors are known to arise in the application of the available regularities to individual cases. These subjects include a great part of biology, psychology, anthropology, history, cosmogony, engineering, economics, and quantum physics. I shall refer to such studies as "irregular subjects"; and the thesis of this article is that scientific explanation is perfectly possible in the irregular subjects even when prediction is precluded. One consequence of this view is that the impossibility of a Newtonian revolution in the social sciences, a position which I would maintain on other grounds, is not fatal to their status as sciences (*3*). Another consequence is the reassessment of Darwin's own place in the history of science relative to Newton's.

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Darwin's Importance

We often confuse three criteria in estimating the importance of the great figures in the history of thought. The first is the indispensability of what they wrote or said, regardless of its effect, judged as a stage in the development of our present beliefs. From this point of view, to earn a place in history, a man need only be the first to discover the material or express the idea in question. The second criterion is their effect on other thinkers, and nonthinkers, which, unlike the first, requires publication and recognition (or misinterpretation). The third criterion is the extent of their personal indispensability. To judge this, we must make some estimate of the time that would have elapsed before the same contribution would have been made by others, had the individual under assessment never existed. If we introduce an index of "lucky fame" as the ratio of a man's importance on the second criterion to his importance on the third, it seems very likely that Darwin has the highest index of lucky fame in history. In fact, what is often regarded as his key contribution was formulated by Wallace before Darwin published it. Admittedly, almost the same calamity befell Newton, but only with respect to his gravitational work; his optics, dynamics, and mathematics are each enough to place him in the front rank. Moreover, Darwin's formulations were seriously faulty, and he appears to have believed in what many of his disciples regard as superstition, the inheritance of acquired characteristics and the benevolence of Natural Selection. Of course, Newton believed that some orbital irregularities he could not explain were due to the interference of angels, but he did achieve a large number of mathematically precise and scientifically illuminating deductions from his theory, which is more than can be said of Darwin. Somehow, we feel that Darwin didn't quite have the *class* that Newton had. But I want to suggest that Darwin was operating in a field of a wholly different kind and that he possessed to a very high degree exactly those merits which can benefit such a field. In place of the social scientists' favorite Myth of the Second Coming (of Newton), we should recognize the Reality of the Already-Arrived (Darwin); the paradigm of the explanatory but nonpredictive scientist.

Let us proceed by examining briefly the attempts by Darwin and others to encapsulate the principles of evolution in the form of *universal* laws and base *predictions* on them; and let us contrast their lack of success in these endeavors with the tremendous efficacy of the *explanations* they produced. During this comparison we shall try to extract the formal properties of the two key types of proposition that are associated with explanations in the irregular subjects: one type of proposition is a weaker relative of predictions, and the other type is a weaker relative of laws.

Hypothetical Probability "Predictions"

The suggestion that in evolution we see the "survival of the fittest" has some well-known difficulties. In the first place,

the definition of "the fittest" is difficult even when made relative to a particular environment. It is fairly obvious that no characteristics can be identified as contributing to "fitness" in all environments. Thus, strength may increase the chance of fighting so much that it decreases the chance of survival, and intelligence may be antiadaptive in anti-intellectual societies. Furthermore, maximum specialization for a particular environment is in general incompatible, morphologically and genetically, with maximum flexibility to withstand sudden environmental changes (*4*). We are inclined to say that the organisms adopting the former line of development tend to be "fitter" *until* the change occurs, and the latter fitter *when* it occurs. Whatever we say, it is quite clear that we cannot predict which organisms will survive except in so far as we can predict the environmental changes. But we are very poorly equipped to do this with much precision since variations in the sun's output and even interstellar influences have substantial effects, quite apart from the local irregularities of geology and climate. However, these difficulties of prediction do not mean that the idea of fitness as a factor in survival loses all its *explanatory* power. It is not only true but obvious that animals which happen to be able to swim are better fitted for surviving a sudden and unprecedented inundation of their arid habitat, and in some such cases it is just this factor which explains their survival. Naturally we could have said in advance that *if* a flood occurred, they would be *likely* to survive; let us call this a hypothetical probability prediction. But hypothetical predictions do not have any value for actual prediction except in so far as the conditions mentioned in the hypothesis are predictable or experimentally producible: hence there will be cases where we can *explain why* certain animals and plants survived even when we could not have *predicted that* they would. And it is a feature of the irregular subjects that, unlike classical atomic physics, the irregularity-producing factors lie outside their range of observation and are not predictable by reference to any factors within this range (*5*).

It should be noted that these "predictions" are not easily falsified by observation, since they only assert the *likelihood* of a certain outcome. Their cash value is thus very much like that of a promissory note which says, "*If* I ever have enough money, I will *probably* pay you $100," whereas an ordinary prediction is like a check for the sum.

A second kind of difficulty with the "survival of the fittest" principle is that many organisms are killed by factors wholly unconnected with *any* characteristics they possess—for example, they happen to be sitting where a tree or a bomb falls. Of course, this is sometimes due to a habit or property they possess; but that is not always true, since even identical twins with identical habits do not always die together. This really shows that (i) even at the limits of stretching, "the fittest" refers to characteristics of an organism, and spatiotemporal location is *not* such a characteristic (in physics, the study of the "properties of matter" covers elasticity and molecular structure but not location), and (ii), location sometimes determines survival. So it is simply false to suppose that "fitness" *universally* determines survival. Of course, one could go a step further and define "the fittest" as "those which survive"; this is not stretching but breaking the concept, and this step would be fatal to all the scientific claims of the theory. We can get by with a tendency-statement instead of an exact law, because it justifies hypothetical and hence occasionally testable predictions and also explanations, but not with a tautology.

H. Graham Cannon is thus entirely mistaken when he says: "So Darwin pointed out that in the struggle for existence it will be those most fitted to survive who do in fact survive. . . . What are the fittest? Simply those that survive" (*6*). Darwin's discovery was that in the world the way it is (and has been), the fitness of the organism, in a perfectly recognizable but complex sense of "fitness" was very often the *explanation* of its survival. In a world where accidents were extremely frequent and mobility was very low, Darwin could never have supported this claim: there would not be enough correlation between the possession of observably useful characteristics and survival to make it plausible. It was partly because the opposing theory of the time was supernatural that insufficient attention was paid to the difference between Darwin's account and other possible naturalistic accounts of the history of life. If good luck in the avoidance of accidents, rather than fitness, was the dominant theme of that history, Darwinism would have been unimportant. And in it there was still the unexplained existence of variations. But Darwinism, like cosmological theories of continual creation, had the added advantage that it spread the inexplicable element *thin*, thus making it scientifically more palatable than a large lump at the beginning, whether the lump be matter or numbers of species. Darwin's success lay in his empirical, case by case by case, demonstration that recognizable fitness *was* very often associated with survival, and that the small random variations *could* lead to the development of species. He did not discover *an exact universal law* but the utility of *a particular indicator* in looking for explanations.

Survival of a Species

In this Darwin was greatly assisted by a feature of the data which constitutes a third difficulty in the attempt to sum up his account under the formula "survival of the fittest," no matter how *fittest* is defined. This is, of course, the fact that our concern is with survival for thousands of generations, not with survival to adulthood for one; and certain factors enter into, or are absent from, an explanation of the form of the *ultimate descendants* of a certain population, by comparison with an explanation of the form of *the adults* in the original population. In particular, we must add the variations in reproductive efficiency (including mating efficiency, where sexual reproduction is involved) and in parental rearing efficiency, as well as genetic variability, and subtract some considerations that affect postclimacteric survival (*7*). However, this transition to what Simpson calls the "differential reproduction of the best-adapted" does not eliminate the effect of the first and second points above; they apply with undiminished force. The theory in this form can deal with a different and more appropriate task; but it is still not capable of generating more than hypothetical probability predictions since both extensive and local catastrophes will play a large part in determining the survivors, regardless of their characteristics. And in the same way as with the simpler version of the principle, though more efficiently, we shall in retrospect still be able to *explain* many features of the record by reference to the characteristics of the surviving animals and the nature of the environmental changes. But we shall not be able to do this always; for there will still be the cases where a whole population, or that subset of it carrying

certain characteristics, will be annihilated in a way that requires and justifies no reference to its adaptiveness, yet makes a substantial difference in the record of life on earth.

At this point one may wish to say that these explanations, too, are part of evolutionary biology. They are certainly part of the history of life on earth, and they are certainly naturalistic explanations. The problem is like that resolved by the great philosopher of history, Collingwood, when he laid it down that the history of man is the history of ideas; we feel that floods and earthquakes have some importance for history, but one can of course discuss their *effects* on man within Collingwood's definition. In our case, we can include such explanations as part of evolutionary biology if we wish, and admit that Darwin's theory and Mendel's additions are not involved or relevant. Or we may omit it and concede that evolutionary theory cannot alone explain the morphology and paleontology that is its field. Which decision we make is not important; but a recognition of the point, however described, is. For we cannot assess Darwin's contribution except by comparing the extent of the domain of his explanations with the domain in which we can and need appeal only to explanations of a kind that Linnaeus (or anyone else who thought the species separately created and by their nature unchanging) would have found perfectly acceptable (*8*).

Considerations Novel to Darwin

When, today, we reach the point where we are discussing a *sequence* of generations in terms of natural selection, we find ourselves faced with a fourth difficulty in any attempt to state exact laws of evolution. It is the first of those we have discussed which involves a consideration wholly novel to Darwin—the idea of random mutations (*9*). Essentially, this is a feature of the theory with a logical character which is the opposite of the catastrophes because the mutations, more or less unpredictably, *add* a new element while the natural accidents unpredictably *subtract* an old one. Again, we can sometimes be sure that the new element is a mutation after it appears—for example, nonalbinism in an albino population. That is, we can explain (in a weak sense here, though with some mutations we can go into details) the phenotypic appearance of an organism by identifying it as a mutation, although we could not have predicted it. Or to be more precise, we might have predicted it, because it does sometimes happen and we might have just had a hunch it was about to crop up. But we cannot give any *rational grounds* for supposing it to be more than a remote possibility that a particular litter from an albino strain will contain a nonalbino, whereas we can be perfectly confident that, when it occurs, it is a mutant, and we can sometimes be confident of the focus of the mutated gene on the chromosome and even of the cause and *modus operandi* of the mutation.

As a fifth and final point, one which does directly contradict one of Darwin's conclusions in the first edition of *The Origin of Species,* we must mention another side of the second point, about "accidental deaths." Just as some organisms and species are exterminated regardless of their characteristics, so some survive despite the handicap of maladaptive (that is, the handicap of antiadaptive and of nonadaptive) characteristics. The "pressure of the environment" is a statistical pressure, and Fisher's proofs of the efficiency of this pressure even on small differences in adaptiveness, being statistical proofs, implicitly allow the possibility that sometimes the unlikely will occur. It is evident from the fossil record that it must have occurred many times, and dynasties have stood when an all-or-none law of selection would have felled them—have stood and have founded a genealogy that would not otherwise have existed. The notion of "random preadaptation," an important explanatory device in neo-Darwinism, relies on just this point. What is true of organisms is true of characteristics, and we have to abandon Darwin's original belief that "every detail of structure in every living creature" has either current or ancestral utility. Not only the mainly nonadaptive form of some antelope horns but some antiadaptive characteristics—either linked genetically with more useful properties, or providing a component for a highly adaptive heterozygote, or by chance alone—will survive for a greater or lesser time, with small and large effects on the course of organic development. The best we can do in the face of such difficulties is to talk of "differential reproduction of the fittest *and the fortunate.*" Yet, here again, as in the case of mutations, we have explanations at hand which have no counterpart in the realm of predictions. We can explain the unlikely outcomes of partially random processes, though we cannot predict them. We are not hard put to explain that a man's death was due to his being struck by an automobile, even when we could not have predicted the event. Now this kind of case does admit of hypothetical probability prediction, but as we shall see, there are cases where not even this sickly relative of ordinary predictions is possible.

The Logic of Predictions and Explanations

It is natural enough that the logic of explanation should appear to parallel that of prediction. Sometimes, in fact, it does. There are specific occasions, particularly in classical physics, when we explain and predict by reference to the same laws. But this is an accident, not a necessity, as it turns out. Put the matter in general logical terms and the similarity still appears to hold: to predict, we need a correlation between present events and future ones—to explain, between present ones and past ones. And who would wish to insist that a difference of tense has any logical significance? As Hempel and Oppenheim say (*2*, pp. 322, 323), "The difference between the two is of a pragmatic character . . . whatever will be said . . . concerning the logical characteristics of explanation or prediction will be applicable to either. . . ." They suggest, plausibly enough, that if we cannot derive the event to be explained from known general laws which connect it with antecedent conditions, we are likely to be deceiving ourselves if, in retrospect, we regard it as explained by reference to those antecedent conditions. And if we can so derive it, then we are in a position to predict it.

Naturally, previous writers on this subject have not overlooked such examples as unpredictable catastrophes being used as the explanation of their consequences. But they have taken the existence of hypothetical probability predictions, which are of course possible in such cases (*10*), to show that the event explained could *in principle* have been predicted. That is, a prediction of the event being explained was possible if we had known, or after we did know, the *catastrophe* was going to occur, but *before* the event. This is a somewhat unhelpful sense of "in principle," since until that day when

everything is predictable, there remains the fact that we can often explain what we could not predict, and surely this feature should be mirrored in any analysis of these notions. Furthermore, there are good grounds for saying we *cannot even in principle* predict everything (uncertainty principle, classical unpredictability of a computer's state); hence, good grounds for saying that even in principle explanation and prediction do *not* have the same form. Finally, it is not in general possible to list all the exceptions to a claim about, for example, the fatal effects of a lava flow, so we have to leave it in probability form; this has the result of eliminating the very degree of certainty from the prediction that the explanation has, when we find the fossils in the lava. But we can go further; we can show, quite independently, a gross logical difference between the two. (There is a large area of noncausal explanation in the sciences in which the two are completely unrelated, but I confine my remarks to causal explanation.)

For when we get down to some exact cases, we do discover something asymmetrical about the two situations, prediction and explanation. What we are trying to provide when making a prediction is simply a claim that, *at a certain time,* an *event* or state of affairs will occur. In explanation we are looking for a *cause,* an event that not only occurred earlier but stands *in a special relation* to the other event. Roughly speaking, the prediction requires only a correlation, the explanation more. This difference has as one consequence the possibility of making predictions from indicators other than causes—for example, predicting a storm from a sudden drop in the barometric pressure. Clearly we could not say that the drop in pressure in our house caused the storm: it merely presaged it. So we can sometimes predict what we cannot explain. But can we ever explain what we could not have predicted, *even if we had had* the information about the antecedent conditions? That is, can we explain when even hypothetical probability prediction is impossible? This seems less likely, roughly because finding causes is harder than finding correlations. Yet it is possible, and, in some areas of knowledge, common. For sometimes the kind of correlation we need for prediction is absent, but a causal relationship can be identified. Although the point is the same, it may be helpful to take an example from a different field.

Retrospective Causal Analyses

If we discover that certain industrial chemicals, frequent abrasion, and a high level of radiation exposure sometimes cause skin cancer, we are in no way committed to the view that cancer *frequently follows* exposure to these irritants. Among the vocations which involve such exposure, cancer may be very rare (although *substantially more frequent* than in other vocations). It is presumed that some unknown conditions such as hereditary predisposition, low perspiration production, or accidental environment factors are responsible for the difference between those who develop cancer and those who do not. Nevertheless, when a middle-aged fisherman comes in to a clinic, his face and hands black from years of ultraviolet exposure, and a growth on the back of one hand is diagnosed as a small carcinoma, the physician who can discover no evidence for the relevance of other known causal factors is in a very good position to assert that the cause *was* excessive exposure to the sun.

The form of this argument, which is so often used by the evolutionary biologist, the engineer, and the historian among others in the non-Newtonian fields, is quite complicated and is best approached by taking a very simple example first. (This corresponds to the example of the barometer which enables one to predict but not explain a storm.) Here, we can explain but not predict, whenever we have a proposition of the form "The only cause of X is A" (I)—for example, "The only cause of paresis is syphilis." Notice that this is perfectly compatible with the statement that A is often not followed by X—in fact, very few syphilitics develop paresis (*11*). Hence, when A is observed, we can predict that X is *more* likely to occur than without A, but still extremely unlikely. So we must, on the evidence, still predict that it will *not* occur. But if it does, we can appeal to (I) to provide and guarantee our explanation. Naturally there are further questions we would like answered if we are research scientists, such as what the particular conditions are that, in this case, combined with A to bring about X. But the giving of causes, and of scientific explanations and descriptions in general, is not the giving of "complete" accounts; it is the giving of useful and enlightening partial accounts. In fact, even the "complete" account merely includes some extra relevant factors—it simply generates even more puzzling questions as to why the *whole* set of factors is sufficient. The search for a *really* complete account is never-ending, but the search for causes is often *entirely* successful, and someone who saw a man killed by an automobile but refused to accept the coroner's statement that this was the cause of death on the grounds that some people survive being hit by a car, does not understand the term *cause.* The coroner is perfectly correct, even though other factors are involved.

Turning to the more general form of the argument, where several causes of X are known, we see that it has the following form:

1) Conditions or events $A, A', A'', \ldots$ sometimes cause X (for example, prolonged sunburn or skin abrasion or some other factor sometimes causes skin cancer).

2) There are some unknown causes of X, but the majority of those cases of X which are preceded by A or A' or . . . are caused by that A.

3) The incidence of X in the population of A's is very small (for example, only a few people in groups receiving the same amount of sun develop skin cancer).

4) A particular individual i is known to have met the condition A, but not the conditions A′, A″ . . . (for example, i has had as much sun as is needed to produce cancer in some people).

From these premises, the only prediction we can make about i is that he will not develop X. Suppose now that:

5) i develops X.

We may now deduce that the cause of this was probably (and sometimes certainly) A. Hence an event which cannot be predicted from a certain set of well-confirmed propositions can, if it occurs, be explained by appeal to them, and there is no "in principle" possibility of predicting 5 from 1 to 4. It is of course true, and trivial, that other data might enable one to predict 5. But I have only wished to argue that the kind of knowledge we do have about evolution enables us to provide well-justified and informative explanations, without predictions.

To go one step further, it is probably not possible to list all the known causes for an evolutionary event such as the extinction of a species; but we do not need to, as long as we can *recognize* them with some reliability. When they are present, we can still identify the

causes of events *after* they happen, without committing the fallacy of *post hoc ergo propter hoc,* which the requirement of predictability-in-principle was designed to avoid.

Careless use of such arguments does produce *ad hoc* explanations; but it is an error to conclude that in general such arguments are vacuous, as do those who think the theory of evolution wholly empty, and thus capable of "explaining" anything. Cannon says, "forty years ago, it appeared to me that orthodox Mendelism . . . was capable of explaining *any* genetical result" (*6,* p. 83); and he regards neo-Mendelism as even more "omnipotent." But he mistakes the explanatory fertility of a theory for explanatory omnipotence—that is, vacuity. If we find a markedly nonbinomial distribution of characteristics in each generation of descendants from a genotypically well-identified pair, we cannot explain this by merely mentioning some *possible* cause. We have to show, as in the cancer case, that (i) this cause was in fact present, (ii) *independent* evidence supports the claim that it can produce this effect, and (iii) no other such causes were present. That this can be done is the mark, and a well-earned mark, of success: in this case, of Mendelism, and in more general cases, of evolutionary theory.

Notice that we do not have to be able to give a law of the usual form of classical physics, a universal functional relationship, let alone a mathematical one. Indeed I prefer to avoid using the term "law" of propositions like 1; they are, logically speaking, particular and not universal hypotheses. However, they can usually be established only by study of a range of cases and hence in some sense might be said to "reflect" a regularity or set of regularities. The logical key to the whole affair is that one can identify a cause without knowing what the conditions are which are necessary for its causal efficacy. When someone says that the explanation of the Irish elk's extinction was the swamping of its habitat, he means that *in the circumstances* this event was sufficient to ensure its extinction, and had this event not occurred, it would have survived. But he would immediately agree that (i) he could not exhaustively specify the circumstances which are essential, although we have in mind the terrain and climate and the animal's weight, hoof size, predators, reproductive habits, and so on; and (ii) there are other possible causes (for example, an invasion of Arctic wolves) which, had *they* been present, would have led to the same effect in circumstances which were in every respect the same except for their presence and the absence of flooding. A more complex but basically similar analysis is required for other cases—for example, the explanation of man's uniqueness, among the bipedal mammals, in running rather than leaping like the kangaroo, in terms of his arboreal ancestors (*12*). These cases illustrate the weakness of talking about "applying a universal law" in order to explain; if you have one, it may be helpful, but if you do not, you may still know a good deal about the possible and actual causes of the events you are studying. Without the universal law, it is not possible to make predictions. The elks might have survived that degree of flooding for all we could produce in the way of laws to the contrary; but if they did not, and nothing else changed, we can reasonably conclude that the explanation is the flooding.

It is not surprising, therefore, that when we turn to the attempts of Darwin and the Mendelians to formulate some laws of the traditional kind, or to make predictions, we find the results to be very unsatisfactory. As Waddington says, even the modern attempt to develop a mathematical approach to evolution has not "led to any noteworthy quantitative statements about evolution. . . . The formulae involve parameters . . . most of which are still too inaccurately known to enable quantitative predictions to be made or verified" (*13*). And if this is the case for the mathematical theory, the case is much worse for exact statements which do not involve the flexibility of mathematical relationships. What can be said is well expressed by Darwin in his autobiography, where he says that *when* there is a struggle for existence, "favorable variations would *tend* to be preserved, and unfavorable ones to be destroyed." Tendency statements like this are explanation-indicators; they justify no more than very weak hypothetical predictions with unspecified conditions ("*if* everything else was the same, then . . ."), for they tell us nothing about the likelihood of conditions of struggle or the strength of the tendency. Perhaps the best way to express their empirical content is to say that they suggest that certain future states of affairs are very *unlikely*—namely, equilibrium of a mixed population *when* there is competition for survival. Indeed Darwin too readily concludes from Malthus' argument that "a struggle for existence inevitably follows," or again, "there must in every case be a struggle for existence, either one individual with another of the same species, or with the individuals of distinct species, or with the physical conditions of life" (*14*). The legitimate conclusion must contain the qualifying terms "eventually" and *"ceteris paribus,"* and a less definite basis for purposes of prediction would be hard to find.

But when we have only such statements, we have a great deal, though we lack much. Often it will be beyond the capacity of a particular subject, such as evolutionary biology or molar psychology, to provide more than this, especially when dealing with past events. Darwin's greatness lay in the use to which he put such statements in explanation, and as he says in the last chapter of *The Origin of Species,* "It can hardly be supposed that a false theory would explain, in so satisfactory a manner as does the theory of natural selection, the several large classes of facts above specified." His work indeed showed that the theory was not *false.* I hope that this study may make clearer why it is not *trivial,* although its principles cannot be precisely formulated, and although it is not committed to any predictions about the future course of evolution, despite Darwin's hopeful voice on the last page of his great work: "We may feel certain that the ordinary succession by generation has never once been broken, and that no cataclysm has desolated the whole world. Hence we may look with some confidence to a serene future of great length." I wish that the great strength of his theory did indeed justify such a prediction. But I fear it is only committed to the view that *if* the struggle for existence continues, the forms of life will *probably* change. Its great commitment and its profound illumination are to be found in its application to the lengthening past, not the distant future: in the tasks of explanation, not in those of prediction.

References and Notes

1. See, for example, R. B. Braithwaite, *Scientific Explanation* (Cambridge Univ. Press, New York, 1953); K. R. Popper, *The Logic of Scientific Discovery* (Hutchinson, London, 1959), p. 59.
2. C. G. Hempel and P. Oppenheim, "The logic of explanation," in H. Feigl and M. Brodbeck, Eds. *Readings in the Philosophy of Science* (Appleton-Century-Crofts, New York, 1953).
3. The grounds are roughly that we already know of crucial variables that are not within

the observational range that defines the science. (See "A possible distinction between traditional scientific disciplines and the study of human behavior," in *Minnesota Studies in the Philosophy of Science*, vol. 1, *The Foundations of Science and the Concepts of Psychology and Psychoanalysis*, H. Feigl and M. Scriven, Eds. (Univ. of Minnesota Press, Minneapolis, 1956).

4. The best advanced discussion of "fitness" with which I am acquainted is J. M. Thoday, "Components of fitness," in "Evolution," *Symposia Soc. Exptl. Biol. No. 7 (1953)* (1954).
5. In quantum physics we envisage the further possibility that there are no such factors, only the irregularity in the individual events, but we have the partial compensation of some statistical regularities. These are in some respects more informative than the nonquantitative probability and tendency statements of psychotherapy, personality theory, psephology, and so on.
6. H. G. Cannon, *The Evolution of Living Things* (Manchester Univ. Press, Manchester, England, 1958).
7. But senile adults may have properties of evolutionary interest—for example, in a gregarious society, especially a gerontocracy. It might seem that we can then include them as environmental conditions for the prospective parents, but this is inadequate (an example is the well-known case of the worker bees). A case where senile maladaptiveness is irrelevant is that of the coiled oyster.
8. The problem of accounting for, for example, the departure of the dinosaurs did not in fact arise until 34 years after Linnaeus' death, with Cuvier's work; but it is too commonly assumed that nonevolutionists would have *had* to assert, as they usually did with the few fossils of extinct forms recognized in the 18th century, that the animals still existed in some as-yet-unexplored part of the globe. They could also have said that a catastrophe that indiscriminately annihilated the life forms in some area was responsible—that is, one of the catastrophes discussed in the second point above. This involves no commitment to evolution.
9. Darwin believed in unpredictable variation, of course, but the several genetic origins of this were not understood by him, nor for that matter were they clear to Mendel.
10. For example, "If there is a volcanic eruption which produces a vast lava stream, then organisms in its path will probably be destroyed."
11. People have sometimes argued that if *A really* is the cause of *X*, it must *always* be followed by *X*. This is to confuse causes with sufficient conditions, and practically to abolish them from the applied sciences, since there are almost no absolutely reliable statements of sufficient conditions available there. Causes are not necessary conditions either; their logical nature is complex, though there is relatively little difficulty in using the term "cause" correctly—a situation which characterizes other fundamental terms in science, such as "probability," "truth," "explanation," "observation," "science," and "simplicity."
12. J. Maynard Smith, *The Theory of Evolution* (Penguin, Baltimore, Md., 1958), p. 245.
13. C. D. Waddington, "Epigenetics and evolution," in "Evolution," *Symposia Soc. Exptl. Biol. No. 7 (1953)* (1954).
14. C. R. Darwin, *The Origin of Species*; these and the preceding passages are quoted by Flew in his illuminating essay "The Structure of Darwinism," in *New Biology* (Penguin, Baltimore, Md., 1959).

Further Reading

The outstanding work on the logical problems of biology, and, in my view, an extremely important book, is Morton Beckner's *The Biological Way of Thought* (Columbia Univ. Press, New York, 1959).

Russian-English Transliteration

An exchange of views on this problem shows that a universally accepted solution is not yet at hand.

Comment by Hamp

The article by Gregory Razran [*Science* 129, 1111 (1959)] on the transliteration of Russian draws welcome attention to our inconsistent practice in a matter where we could readily do better. I can only applaud Razran's sensible attitude and second his call for improvement. I think, however, that we can clarify the problem further, and understand in some measure the present confusion, by raising a point of principle which Razran does not touch.

It has taken a fair part of the last half century for workers in linguistics to appreciate clearly the fundamental distinction that must be drawn between speech and writing. A glance at any of the modern textbooks on linguistics will amply illustrate this. Linguists are still all too conscious of the fact that the purport of this finding has in many respects not yet been brought home to the literate public at large, which includes their fellow scientists.

For our present purpose, this distinction means that the graphic system used in a particular culture area (a specific subtype of Cyrillic, in this case) is not identical with the phonemic system of a particular language (Russian in this instance). Indeed, the two can be analyzed quite independently. Only in rare instances are the two systems nearly congruent (Finnish is such a case), so that the distinction may be ignored altogether. In addition, we must remember that there are, too, the graphs of the target culture (a subtype of West European Roman, in our own case) and the phonemes of the target language (American English for us). There are, then, four separate systems in play, whose useful combinations we must now consider.

No one, presumably, is interested in matching Cyrillic graphs directly with English phonemes—that is, devising an arbitrary way for reading off a line of printed Russian with a thoroughly English accent. We get that result without strain from the less apt students in a Russian class. (The question is not idle, however, in principle; Egyptologists must decide how to cite forms intelligibly to one another aloud, even though they can scarcely guess at all what a large portion of the language sounded like.) Similarly, we have no immediate use for Roman letters with a Russian accent, unless perhaps we are training actors. There is very great use for comparison of Russian phonemes with English phonemes; that is what a linguist must consider in designing adequate and efficient teaching materials—both for Russians and for Americans. Finally, there is the problem of matching Cyrillic graphs with our Roman graphs; we will call this task "transliteration," *sensu stricto*.

At one point (page 1111), Razran says: "The rationale of the practice is presumably that of facilitating library cataloging and filing by indicating that the English combinations of letters correspond to single Russian letters. But, plainly, this limited and doubtful advantage must be pitted against the fact that ligatures and extra capitals are both expensive and unesthetic, add nothing from the standpoint of approximate pronunciation, and, indeed, have hardly ever been maintained consistently." Consistency is something which, like Razran, we all hope for, but which the linguistic engineer cannot enforce. Expense and esthetics are problems apart, and we must consider them judiciously in turn. But the "limited and doubtful advantage" of unambiguous transliteration is a matter of considerable concern to a

fair number of consumers—for example, the librarians to whom automatically convertible alphabetization is important. It should be emphatically noted that efficient transliteration can be made and be used without any knowledge of pronunciation (that is, phonemics) at all. This point is worth making, quite apart from Razran's statement, since it is a point that linguists, whose prime interest understandably is spoken language, tend characteristically not to be at pains to underline for the benefit of nonlinguists.

Yet another task may fruitfully be considered. We may take Russian phonemes (*not* Cyrillic graphs), assign them values (in whatever symbolic system we please; linguists observe technical conventions on this point, which need not detain us), and then transcribe these values into suggestive Roman graphs. We call this operation transcription. This is clearly what Razran means (page 1113) by "a discriminating use of English as is."

Transcription is obviously different from transliteration, and Razran is in effect collapsing this distinction when he says (page 1113): "The objective of any system of transliteration is obviously to convey to the reader as closely as possible the phonetic value of the transliterated material." The system which he proposes, laudable as it may be, falls far short of phonetic (or better, phonemic) adequacy—and I am not thinking here of the requirements of technical linguists but of those which the layman may reasonably lay down. For example, not only is the place of stress important—even crucial—in Russian, but the values of the vowels are very different in stressed and unstressed syllables. Any attempt to stick close to the Cyrillic graphs will fail to bring this out.

In short, with Russian as with many written languages, one cannot serve two masters simultaneously—graphs and phonemes—and produce the results desired and often claimed. Some of the confusion deplored by Razran results from just this; but Razran's proposal itself does not really come to grips with the basic problem. Instead, he simply decides in advance which aspects he thinks most worth conveying.

A number of alternatives are conceivable. One would be to have two completely different, but generally accepted, conventions: For newspapers and what we might call colloquial uses, we could settle on something admittedly makeshift, but plain and manageable, such as Razran has set forth. For more technical uses we could agree on both a transliteration and a transcription, each of which would be accurate and scientifically based, if admittedly a trifle "unesthetic" in places. Competent persons could then decide whether for a given purpose (for example library catalogs as opposed to certain technical manuals) the transliteration or the transcription was more valuable and effective. For a few special purposes (for instance, names occurring in geographic literature, where both oral-aural and visual recognition are desirable) the cumbersome but unambiguous method of writing the form twice could be used—the transliteration and the transcription, with the two separated, say, by a slant line. Rules of capitalization need apply only to the transliteration.

There would be, of course, for each written language as many transliterations as there are other (target) graphic culture areas and as many transcriptions as there are other languages (target standard phonemic systems), the latter in turn being subdivided in a few cases (as in Serbo-Croatian), where more than one graph system serves the same language.

It would not be at all difficult for competent linguists to design workable standard systems of transcription and transliteration. For example, Table 1 gives a possible consistent transliteration system which is relatively free of diacritics.

A reasonable approach to a satisfactory transcription system would take more technical discussion than is appropriate here.

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Table 1. A possible system for transliterating Russian into English.

Russian	English	Russian	English	Russian	English
а	a	к	k	х	kh
б	b	л	l	ц	c
в	v	м	m	ч	ch
г	g	н	n	ш	sh
д	d	о	o	щ	shh
е	e	п	p	ъ	”
ё	ë or ö	р	r	ы	y
ж	zh	с	s	ь	’
з	z	т	t	э	è or ə
и	i	у	u	ю	ü
й	j	ф	f	я	ä
		ѳ	ph		

Comment by Fabergé

Razran regrets the existence of seven or more systems for transliterating Russian into English and proceeds to contribute one more such system. Any conceivable transliteration is at best a crude compromise between exactness, simplicity, expediency, and typography, and it does not seem to me that the new proposal has any marked merit, on balance, over several of those already in current use; thus, its introduction can only add to confusion.

The requirements of bibliographic cataloging and of the daily press are different; in the latter case it may be good to use rough English phonetic equivalents, so that approximate pronunciation is achieved at sight. In bibliographies phonetics are not the primary requirement, but some attention must be given to etymology. Many languages using the Latin alphabet have very divergent phonetic values for letters, yet in practice there is no requirement for transliteration, mispronunciation notwithstanding. Thus, few Americans would pronounce *all* of the following correctly: de Broglie, Brouwer, Cajal, Chasle, Fresnel, Hammarsten, James Clerk Maxwell, Perrin, Szasz, Zernike. I do not know the "correct" pronunciation of "Joule" (10 million ergs) or of "Demoivre's theorem," since both discoverers lived in England but were of French origin. Luckily, no one has yet proposed that any of these be transliterated.

Systems which are based on a related language that uses the Latin alphabet, such as Czech in the case of the *Mathematical Reviews,* and which give some attention to etymology, result in a measure of consistency and are of fairly wide applicability. They are not confined to English. The new proposal, on the other hand, is purely Anglocentric and would

be most inconvenient to, say, a German or a Frenchman. In saying that *Pawlow* and *Pavloff* are quite obsolescent, Razran is merely giving the current (and far from obsolescent) German and French variants, respectively, of what is essentially his own system in these two languages. English is one of the worst bases for phonetic transcription, because of the multiplicity of phonetic values of almost all letters: it will be sufficient to remember Bernard Shaw's word for fish, "ghoti."

There are also some specific points where the new proposal seems unacceptable.

In my judgment the aspirated English *h* (as in *hat,* pronounced by a non-Cockney) is a very close approximation to the Russian **х** (though not to the Ukranian pronunciation); this *Mathematical Reviews* correctly recognizes. The spelling *Hrushchev* yields at once a fair approximation, while *Khrushchev* merely forces the Englishman to produce an inhibited pout, followed by a sneeze. In reverse, Russians themselves usually transliterate *h* as **г**, but the reasons for this are historical rather than phonetic.

It is true that the genitive **-го** sounds like **-во**, but to anyone knowing Russian, the proposal to transliterate this invariably as *-vo* is shocking because it looks like an illiterate error of grammar.

The use of *ye* for **е** is often phonetically correct, but not always; it would, moreover, result in names that begin with the same letter in Russian appearing near the opposite ends of alphabetical lists. To make invariably *k* equal to **к** is also questionable. I spent a frustrating hour in one university library trying to find the *Doklady* of the Academy of Sciences of the U.S.S.R. In vain I tried *doklady, doclady, comptes rendus, proceedings, academy, academie, nauk, St. Petersburg, Petrograd, Leningrad, U.S.S.R.,* and *Russia*; I was then told that no such publication existed, but finally found it under *Akademia.* Why not "Akadaymee day Sians do Parry?"

The proposal to distinguish between Russian and Polish names by using *-sky* and *-ski,* respectively, is impractical, since many "Russian" names such as Lobachevsky and Tchaikovsky are in fact of Polish origin, as is obvious to a Russian.

To have any phonetic value at all, a transliteration must include the position of the stress. Russian tonic accent follows no rule and has to be learned arbitrarily; Russians themselves may not know whether a man chooses to call himself Ivánov or Ivanóv.

Strict adherence to some system would produce more confusion than it removes in the case of names of Russians whose main scientific work has been published in foreign journals and who are consequently already cited in many bibliographies with the corresponding spelling—for example, Markownikoff, Tschitschibabin, Tschetwerikoff.

It seems to me that any conceivable new transliteration system can result at best in negligible improvements, and that it is far better to avoid confusion and to take one of the extensively used systems, imperfect as they are. Above all, the practice of giving literature citations and making library cards in the Cyrillic alphabet should be encouraged. It takes little more time to learn the letters than to master some one of the transliteration systems, and many rather monstrous difficulties are thus avoided. These problems are not confined to Russian; readers may be amused to look at the last two pages of the preface to T. E. Lawrence's *Seven Pillars of Wisdom* to see the much greater difficulties encountered in Arabic, as well as that author's very sane attitude toward transliteration.

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Comment by London and London

The system of Russian transliteration presented by Razran contains, to be sure, certain simplifications. Unfortunately, it also presents certain insuperable difficulties for the cataloger who is not in full command of the Russian language—namely, with regard to the transliteration of **е** and **ё** as *e* or *ye* and *o* or *yo*, respectively. The fact is that the dieresis over the **ё** is almost never indicated in Russian publications and the conscientious cataloger would have to consult a dictionary or even a Russian-speaking person in every case to determine whether *e, ye* or *o, yo* were indicated. (There are, moreover, no easy rules for such determination, as many beginning students of Russian have discovered.)

For the non-Russian cataloger even the transliteration of the genitive endings **-ого** and **-его** as *-ovo* and *evo*, respectively, following Razran's suggestion, cannot be entirely automatic. The cataloger must be able to distinguish genitive from nongenitive endings having the same spelling but retaining the sound *g*. The words **много, немного, строго,** and **убого,** for example, must all have the transliterated ending *-ogo,* not *-ovo.* This is not to mention, of course, a word such as **благо,** ending in **аго,** which must be transliterated as *blago.* The cataloger must also be careful to distinguish between the **г** which is pronounced as *v* in the *middle* of the word **сегодня,** and the **г** which retains the sound *g*—for example, in the middle of the word **снегопад.**

As to the value of any system of transliteration in itself as an aid to pronunciation—and to Razran this is *the* obvious function of any system of transliteration—it is doubtful that any foolproof scheme can be devised. While Razran's proposed substitution of *y* for **ї** in the transliteration of **й** may represent an improvement in certain instances, it is actually misleading in others. For example, how many would suppose that the transliterated word *day* (formed according to Razran's system) for **дай** is pronounced like *die*?

We may also note that, while the daily press may have evolved in some instances a more uniform system of transliteration, this system is not necessarily better geared to correct pronunciation. Otherwise, we should be reading (and hearing) *Khrushchov* (according to Razran's system) or *Khrushchóff*, or even *Khrooshchóff* (if one is really going to be serious about getting the name down right).

Actually, is not guaranteeing approximate pronunciation of transliterated items in the *scientific* literature a dispensable luxury? For example, how many of us can correctly pronounce French, Italian, or Polish references, which are already in the Latin script? The Library of Congress system of transliteration, with its uncomplicated one-to-one correspondence between Cyrillic and Latin symbols, serves the cataloging function admirably and, at the same time, makes possible easy recognition of the original Russian by qualified persons. With the exception of the ligatures —which could readily be eliminated— it would appear simplest to retain this system intact and recommend its universal adoption in the *scientific* press.

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Comment by Ray

Razran's article once more demonstrates that the last word will probably never be said on the subject of transliteration.

Fortunately, the summary concisely states the premise upon which the suggestions proposed in the article are based. "The objective of any system of transliteration is obviously to convey to the reader as closely as possible the phonetic value of the transliterated material." It is precisely this assumption, made also by many others in the thorn-strewn field of transliteration, which has led to difficulties. The assumption as stated is unworkable.

We must start afresh. One must redefine the problem of transliteration in such a way as to exclude from it the task of teaching the pronunciation of Russian, or of any other language which is normally written in other than Latin characters. It has been amply demonstrated by contemporary American descriptive linguists that the teaching of a spoken language is a task which presupposes a phonemic analysis of that language. There is no necessary correlation between the phonemic structure of a given oral language and the writing tradition in use by the speakers of that language. The relationship between a spoken language and the writing tradition of the corresponding written language is inevitably historical rather than synchronic. A further elucidation of this point is beyond the scope of the present remarks.

What, then, should "transliteration" aim at? It is here proposed that the objective of a transliteration system should merely be that of providing a means of letter-for-letter substitution in passing from one script to another. This is even implicit in the etymology of the verb *trans-literate*. Ideally, an unambiguous one-to-one correlation of symbols would be desirable.

Obviously a one-to-one correlation is impossible between a writing system with 26 symbols and one with the 32 symbols of the Russian Cyrillic script, as reformed in 1917. Hence, in certain instances a group of two or more Latin letters must be designated as equivalent to a single Russian letter. Failing that, the only other way to make 32 letters out of 26 letters would be to mark at least six of the 26 in some distinctive way. Transliteration systems for Russian, of course, do not use the available Latin letters *q* and *w*. Some less commonly known systems use *j* and *x*. Although the use of diacritics goes contrary to the writing tradition of the English language, it occurs in the system of the *American Slavic and East European Review*, where *ž*, *č*, and *š* are merely the adaptation to Russian of the normal phonetic significance of *ž*, *č*, and *š* in Czech, Croatian, and Slovenian. As such, the system of the *American Slavic and East European Review* has much to recommend it. At any rate, once the problem is redefined as one of expanding a 26-sign system into a 32-sign system, we may proceed.

Now as a practical fact, in performing this expansion we are indeed, whether we wish it or not, constrained by the writing traditions of both the English and the Russian languages. And at the same time, in designing a transliteration system for the two, we have only the most marginal concern with the present-day pronunciation of spoken English and spoken Russian. It is here suggested that the Library of Congress transliteration system, once its objective is understood, adequately fulfils the requirement of a one-to-one correlation between the Russian Cyrillic alphabet and certain conventionally established letters and groups of letters of the Latin alphabet. A very important function of transliteration is that of making possible conformity to the limitations of more generally available type fonts. Hence ligatures are to be avoided. The Library of Congress system uses ligatures with only three combinations: *iu* for ю, *ia* for я, and *ts* for ц. The Library of Congress system does not use any ligatures over the combinations *zh*, *kh*, *ch*, *sh*, and *shch*.

It is in the matter of vowels that there is the greatest need to adhere rigidly to the symbol-for-symbol correlation principle. We may not say "one-to-one" because the Russian Cyrillic script, as reformed in 1917, uses twice as many letters to represent Russian vowels as are available to us in the five Latin letters *a*, *e*, *i*, *o*, *u*. We have somehow to represent а, е, и, й, о, у, ы, э, ю, and я. The solution to this problem has become confused with the fact that palatization is a phenomenon which pervades the phonemic structure of modern spoken Russian, as well as that of the Old Church Slavic language, from which the writing tradition of Russian developed.

We may expand the facilities of the Latin alphabet for representing Russian vowel letters by designating the Latin letter *y* as equivalent to one of the Russian letters. Casting about for the equivalent to be assigned to Latin *y*, the perhaps somewhat irrelevant choice of the Russian ы has frequently been made. This comes to us from the writing tradition of two Slavic languages which normally use the Latin script—namely, Polish and Czech. So long as we do not alter the equation ы = Latin *y*, we avoid confusion. Preoccupations with the function of the Latin letter *y* to designate a "semivowel" belong in the realm of the phonetics and phonemics of the English language and should be excluded from consideration in designing a transliteration system for the Russian Cyrillic script. Any other uses for the Latin *y* involve us in the familiar dilemmas which beset those who are forever tinkering with Russian transliteration. For example, in the article under discussion, the following multiple functions are proposed for the Latin letter *y*: (i) as an equivalent for the Russian Cyrillic letter ы; (ii) as an equivalent for the Russian Cyrillic letter й; (iii) as a member of the two Latin-letter digraphs which must of necessity be fixed to designate the two Cyrillic letters ю and я; (iv) sporadically, when the palatalizing vowel phoneme of spoken Russian /e/ occurs in its allophones [ie] or [io]. Those who plead for rigid adherence to the simple equation ы = Latin *y* have in view merely the avoidance of these pitfalls. They are not concerned with whether the resulting transliterated Russian is "likely to be disyllabized in accordance with English usage." The way to learn to pronounce Russian is to study Russian with a competent teacher, not to make futile stabs at pronouncing a transliteration system.

What, then, shall we do about ю and я? If we set up the equivalents *ju* and *ja*, we invoke the writing traditions of German, Dutch, the Scandinavian languages, Italian, and those Slavic languages which use the Latin alphabet. While this may do for the readers of the *American Slavic and East European Review*, it manifestly runs counter to the linguistic habits of the average American reader of publications devoted to the physical sciences. Probably no speaker of English will object to the equation и = *i*. If the Latin digraphs *yu* and *ya* are denied to us as fixed equivalents to ю and я for the reasons stated above, let us examine the suitability of *iu* and *ia* (with ligatures).

Unfortunately, due to the influence upon Russian itself of the writing tradition of Western European languages, *ia*

(without ligature) may not be used as an unambiguous equivalent for **я**. We have, for example, loan words like **материал** and **потенциал** which, with their derivatives, are of fairly frequent occurrence in Russian scientific literature. It is to conserve the symbol-for-symbol correlation principle, and not out of any desire to embarrass printers, that the Library of Congress has set up the equivalents *iu* and *ia* (each with a ligature) for **ю** and **я**, as distinguished from a possible **иу** and **иа**. On the same principle, the ligatured digraph *ie* is used, when necessary, to transliterate the obsolete letter **ѣ**, which occurs in Russian publications printed before 1917. Again, this use of *ie* (with ligature) has no reference to pronunciation; its only purpose is to make possible the exact restoration of the original Russian unreformed orthography where it occurs.

Fortunately, there is a simpler solution of the irksome **ю** and **я** problem. We may simply ask our printer to supply the not-unavailable letter ĭ. This is possible because of the principle of complementary distribution. It happens that in written Russian there are no instances of the occurrence of **йу** and **йа**. Consequently, when the Latin-letter digraphs *ĭu* and *ĭa* occur, we may invariably know that they designate the *single* Cyrillic letters **ю** and **я**. On the contrary, when the Latin-letter combinations *uĭ* and *aĭ* occur, we may be sure that the original Russian will have *two* Cyrillic letters, **уй** and **ай**, in every case. Thus, the common word **хозяйство** can be unambiguously transliterated *khozĭaĭstvo*, rather than *khoziaĭstvo*, as in the Library of Congress system.

Again, we may apply the symbol-for-symbol principle in fixing an unequivocal transliteration for the Russian Cyrillic letters **е** and **э**. Let cyrillic **е** under all circumstances be transliterated by Latin *e*. Granted that the Library of Congress *ė* is generally unavailable at the printer's for **э**, we may use *é*, *è*, or even *ê*, provided only that a distinction is somehow contrived. If these equivalents offend the sensibilities of readers of French, we may use *ĕ*.

There now remains only the problem of **ц**. Those accustomed to the German and west Slavic writing traditions for the Latin alphabet will find that the equation **ц** = *c* presents no difficulty, but this admittedly will not do for most Americans. From the point of view of pronunciation it is quite unnecessary to show any distinction between **ц** and **тс**. But here again, a transliteration problem is not necessarily related to pronunciation. In Russian orthography, the choice between **ц** and **тс** is governed by etymology, not by pronunciation. **тс** occurs when a word normally ending in **т** has added to it a suffix beginning with the Cyrillic letter **с**. Two common cases may be mentioned. First, the adjectival ending **-ский**, and second, the ending **-ся** for the reflexive of finite verbs. Thus, we have: the following three examples for the first case: **брат, братский** (not **брацкий**) for *fraternal*; **совет, советский** (not **совецкий**) for soviet (as an adjective); **азиатский** (not **азиацкий**) for *Asiatic*. For the second case, we have **являет, является** (not **являеця**) for *is*; **определяет, определяется** (not **определяеця**) for *is determined*; and **вызывает, вызывается** (not **вызываеця**) for *is caused*.

On the contrary, instances where **ц** occurs, but never **тс**, are, for example, original Slavic words, like **овец, яйцо, солнце**, and **отец** and loan-words from Western European languages, representing a *-tion* ending in French or English. For example: **ионизация** (not **ионизатсия**) of *ionization*; **авиация** (not **авиатсия**) for *aviation*; and **организация** (not **организатсия**) for *organization*. Consequently, an English-speaking person will require some knowledge of Russian if he is to know when a transliteration *ts* represents **ц** and when it represents **тс**. In the transcription of titles for bibliographies, and especially in preparing catalog entries for library filing, we can ordinarily assume no expert knowledge of Russian on the part of clerical assistants or even users of the bibliographies or card catalogs. Consequently, the symbol-for-symbol substitution principle is employed, wherein *ts* (with a ligature) invariably represents an original **ц** and *ts* (without ligature) represents an original **тс**. Since the character with the ligature is not generally available, *ts* seems to be a practical substitute.

The only sound guide in devising a workable, unambiguous transliteration system for any language, including Russian, is a rigorous letter-for-letter, or symbol-for-symbol, substitution. The objective sought is a system which will make possible accurate reconversion to the original non-Latin script for positive identification. In the case of Russian, these objectives can be realized with minimal use of diacritics, by using *ĕ*, *ĭ*, and *ts*. The Library of Congress transliteration system may then be followed, with the following simple and obvious variations: **э** = *ĕ* instead of *è*; **ц** = *ts* instead of ligatured *ts*; **ю** = *ĭu* instead of ligatured *iu*; **я** = *ĭa* instead of ligatured *ia*.

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Razran Replies to Critics

Specifics of Russian-English Transliteration: Reply to Hamp

Hamp's theoretical discussion of the linguistics of transliteration as such is of course very welcome, and is well taken. However, with respect to the specifics of Russian-English transliteration, he overlooks the following: (i) the high correlation between the Russian phonemic and graphic systems (much more like that in German and Spanish than like that in English and French) which permits, within limits, the coalescing of "transcription" and "transliteration" (I prefer "phonemic and graphic transliteration"); (ii) the relative simplicity of the Russian phonemic system and its *comparative* closeness to the English one (it is certainly simpler and closer than the Celtic or Arabic system), characteristics which point to relatively simple substitutive means of representing it by the modal phonemic values of English graphs; (iii) the near-ubiquity of the transliterated material in printed English media, which makes economy and esthetics cardinal considerations; (iv) the linguistically nontechnical and operationally limited nature of 99.9 percent of the needs of those who use the transliterations, which contraindicates exacting precision; and (v) the traditions or habits which underlie existing practices and the psychologist's concern with efficiency and with avoidance of engrams that are not readily realizable. Serving "two masters simultaneously — graphs and phonemes" is indeed the intent and, I contend, the achievement of my proposed, not particularly original, system.

Enlargement and elucidation call for structuring the problem and adducing concrete examples. The total consumer population for Russian-English transliteration will, accordingly, be divided into three classes: (i) the technical linguist who for his technical purposes requires complete phonemic *and phonetic* analyses of Russian speech and

writing, but who constitutes presumably no more than 0.1 percent of the consumer population; (ii) the general English-reading and -speaking public, ranging from the most exact nonlinguist scientist to the barely literate layman, who needs no more—and really can use no more—than a pronunciation that is *constant for English speakers* and is reasonably close to that of Russian speakers or, indeed, of any speakers not too far removed from English and Russian phonetics (the Russian and English readers and writers who initiate the consuming of the transliteration and the editors concerned with uniformity and conflation might be regarded as special groups serving and supervising the needs of the general public); (iii) the cataloger, who should be able to function faultlessly with absolutely no auditory or vocimotor involvements and who may even be a deaf-mute. The proposed system satisfies fully the needs of the consumers of the second and the third classes; what the consumer of the first class really needs is a phonetic transcription rather than a more exact—if more exact it can be—phonemic one. Concrete examples follow.

If **Павлов** is transliterated as *Pavlov,* each of the six Russian phonemes is detectably different from its English correspondent: the initial *P* is not aspirated, the final *v* is unvoiced, the four consonants are duller and involve some lip thrusting, and the two vowels have some special characteristics. Yet it is obvious that the pronunciation of *Pavlov* will be quite constant among English speakers and, despite the minor interphonemic differences, the word as pronounced will probably be quite recognizable to Russians. But suppose **Толстой** is transliterated as *Tolstoj* and, let us say, **Чубяшец** as *Čubjašec.* Most readers will undoubtedly use the wrong final phoneme in the first case and be totally confused with respect to the second; there will be no consistency of phonemic usage, and there will be little interconsumer auditory recognition and communication. Yet when the two names are rendered as *Tolstoy* and *Chubyashets,* the transliteration assumes the phonemic adequacy of *Pavlov,* just as *Pavlov* changed to *Pawlow* loses it. Now consider **быт** and **бой,** transliterated as *byt* and *boy,* containing two different Russian graphs (**ы** and **й**) transliterated by the same English graph (*y*); this, on the surface, would seem to make the system unusable for consumers of the third class. But **й** occurs in Russian only after vowels, while **ы** is found only after consonants and never at the beginning of words; the complementary distribution of the two Russian phonemes resolves the phonemic-graphic conflict of the system. Similarly, when (i) **Есенин** and **Андреев** are transliterated as *Yesenin* and *Andreyev,* with an extra English graph for the initial Russian **е** in the first word and for the second, syllable-initiating **е** in the second word, and when (ii) no extra English graph is allotted to **э,** as in transliterating **энергия** by *energiya* (Greek, ἐνέργεια), the positional differential makes the system usable by any cataloger. Each of the remaining 31 Russian graphs in my table is matched by a specific English graph, in some cases, of necessity, by more than one.

(Genitive **-го** transliterated as *-vo* and **ё** with the dieresis omitted, as it usually is in texts for adults, are two cases, and the only two, where Russian-English phonemic transliteration is impossible without a knowledge of Russian—impossible of course in *any* devisable system. However, this irremediable shortcoming obviously affects neither the initial consumer of the transliteration, who knows Russian, nor the general reader who consumes the final product, and it presents really no *extra* problems for the cataloger who has no knowledge of Russian and who of necessity will transliterate the aforementioned Russian graphs by *g* and *e.* All it really means is that in two single instances such a cataloger cannot provide complete phonemic information.)

Hamp states: "For newspapers and what we might call colloquial uses, we could settle on something admittedly makeshift, but plain and manageable, such as Razran has set forth. For more technical uses we could agree on both a transliteration and a transcription. . . ." I do not know what he means by "colloquial uses" and do not care to analyze the semantics of "makeshift, but plain and manageable." But do the Russian-English transliteration needs of the readers of the *New York Times* differ from those of readers of the *Nuclear Review Abstracts*? And isn't the area of "technical uses" very small indeed? In another place Hamp says: "For example, not only is the place of stress important—even crucial—in Russian, but the values of the vowels are very different in stressed and unstressed syllables." But are the vowels so "very different" and is the stress "crucial" to a phonemic transcription? Are the changes, significantly characteristic of only some of the Russian vowels and varying considerably in different regions, more than phonetic shifts of a type common in English and even more in Portuguese? [Xoroʃó], [xʌrʌʃo], and [xərəʃo] are quite interchangeable and intercommunicable in Russia.

Hamp does not offer a phonemic "transcription" of his own, saying only "It would not be at all difficult for competent linguists to design . . ." one. However, the alphabet in his "table of transliteration" is quite puzzling. It is not that of modern Russian, including as it does the discarded **ѳ,** and it is not that of pre-1917 Russian since it lacks **і, ѵ,** and **ѣ.**

Finally, I would like to suggest four minor additions to my proposed system: (i) transliterate **г** before **е** and **и** by *gh*; (ii) insert a hyphen between transliterated **ы** and succeeding **у** (vowels other than **у** either rarely occur after **ы** or present no problem), and also a hyphen between transliterated **т** and **с** (to distinguish **тс** from **ц**); (iii) transliterate **г** before voiceless consonants by *kh* and, perhaps, also **ч** before **н** by *sh*; (iv) transliterate **ё** by *yo* after **зж** and **жж,** leaving *o* for **ё** after **ж** that is initial or preceded by other letters. And then there is the problem of transliterating **Хрущёв.** The common transliteration *Khrushchev* is phonemically inadequate. It should be *Khrushchyov,* as the proposed system would have it.

My original article contained six typographical errors (page 1112): (i) **э** should be substituted for **з** in line 1 of the last paragraph of column 1; (ii) (*Sovetskiĭ*) for (*Sovetskii*) in column 2, line 12; (iii) **ё** for **ė** in column 2, line 24; (iv) Novyĭ for Novyi in column 3, line 5; and, in Table 1, column 2; (v) "Genitive **-го** = *-vo*" for "Genitive **-го,** *-vo*" in line 4; and (vi) *yo* and *o* for **уо** and **о** in line 8.

Addendum: Reply to Fabergé, London and London, and Ray

My reply to Hamp, answers also, I think, the relevant animadversions in the other letters—London and London's concern with genitive **-го** and with **ё** without dieresis, Ray's concern with the multiple use of *y,* his "redefinition" of *transliteration,* and his all-too-lengthy discussion of **тс** versus **ц,** which may be rendered merely as *t-s* and *ts,* as discussed above. Likewise, if an English

graph is chosen to transliterate a Russian one, its *separate* phonemic value is supposedly retained also in combinations of English graphs (this in reply to the *ay* query of London and London): literate Americans rhyme *Adenauer* with *Eisenhower* despite the *au*, and they do not silence *P* in *Pskov*, *k* in *kniga*, *Knobel*, or *Knut Hamsun*. Space forbids treatment of the large portion of irrelevant material in the letters—for example, the comments that English readers may as well mispronounce transliterated Russian words since they mispronounce French words; that a phonemic transliteration is no substitute for a competent Russian teacher (Ray); and Fabergé's strange logic in stating that и does not equal *k* because in "one university library" he could "find the *Doklady* of the Academy of Science of the U.S.S.R." listed only under *Akademia* and not under *Doklady*. (Let him try to find the *Proceedings of the National Academy of Sciences* under *Proceedings*—and, incidentally, no library system uses *Akademia*, only *Akademiia* or *Akademiya*; Fabergé's entire letter teems with irrelevancies and inaccuracies—for example, his remarks about the difficulties of transliterating Arabic and about the use of the Cyrillic alphabet, his comment that my proposal is Anglocentric, that x equals *h*, and so on.)

The writers of the letters are surely behind the times in their unawareness of what is currently being done in the ever-increasing Russian-English translation and abstracting programs. Consultants Bureau and the Pergamon Institute, the chief translation agencies for the physical and biological sciences, do not use the Library of Congress system and through the enforcement of their own system contribute greatly to uniformity, while the *Current Digest of the Soviet Press*, published by the Joint Slavic Committee of the American Council of Learned Societies and the Social Science Research Council, does not use the system of the *American Slavic and East European Review* but one whose "aim is to approximate Russian sounds without diacritical marks" (as stated in each issue of the journal). Plainly, what is needed is (i) greater and speedier efforts to achieve unity and (ii) a realization that, with respect to Russian, phonemic and graphic desiderata are reconcilable (as manifested in the fact that my proposed phonemic-graphic system differs but little from the Consultants-Pergamon graphic system, on the one hand, and from the *Digest's* system, on the other, and is really a compromise or synthesis of the two, though I have been using it for almost 30 years in about four-score publications). Moreover, (iii) the entire matter is experimentally testable. For some time I have been asking English readers to read Russian material in my transliterated system, and on occasion have had the transliteration done by assistants whose knowledge of Russian was derived solely from my table of transliteration, in front of them. Almost always I have found the readers' pronunciations phonemically adequate (except of course for the irremediable x–*kh*, ы–*y* differences, and occasional difficulties with *zh*) and the assistants' transliterations errorless (except of course for corrections of their genitive -го and ё without dieresis). My judgment of degrees of phonemic adequacy may be disputed as subjective; yet, pronunciations could, obviously, be recorded and submitted to a panel of experts for consensual judgment. Let the systems, then, be put to a verifiable experimental test, let a choice be made upon the basis of objective evidence free from habit-bound and ego-involved opinion and conjecture, and let there be unity.

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News of Science

President and Congress Act on Appropriations

Appropriations for many federal departments doing scientific work are being rushed through Congress under the pressure of a move for adjournment by early September. Appropriations bills for the Department of Defense and the Department of Health, Education, and Welfare went to the President recently after being cleared by Congress. The Atomic Energy Commission and a number of smaller agencies also are now learning how much money they will have for fiscal year 1960.

The money bill for defense, which the President signed on 18 August, calls for $39.2 billion. This amount represents a compromise between the Senate bill, which authorized $39.6 billion, and the House bill, which authorized $38.8 billion. The final appropriation, which was cleared by the whole congress after conference, was almost $20 million short of the amount the President requested in his budget message at the beginning of the year.

For research and development, the bill authorizes more than $1 billion each for the three services, with the Air Force receiving the largest amount, $1.16 billion. The Advanced Research Projects Agency, the organization that sponsored the Atlas communications satellite last December, has an appropriation of $455 million. In addition, the Defense Department was given an emergency fund of $150 million, bringing the total figure for research and development activities to $3.8 billion.

HEW Funds Increased

On 14 August, the President signed the appropriations bill for the Department of Health, Education, and Welfare. This bill, sent to the White House 30 July, appropriates $3.446 billion, $282 million more than the President had requested. Almost all of the increases over the President's budget requests were made for health and education programs. The National Institutes of Health will receive $400 million; the Office of Education $431 million; and the Public Health Service $828.9 million. Following a well-established pattern, the funds for the NIH were increased by $105.7 million beyond the amount the President had asked.

In another action on appropriations, the Senate sent to conference a revised

bill which would give the Atomic Energy Commission $2.68 billion for fiscal 1960. The bill must be reconciled with a House bill which appropriated $51 million less. Both bills provide less than the Administration requested, $2.69 billion. The Senate, which accepted the recommendations of its appropriations committee, called for the restoration of funds which the House had cut. The committee report said that the full budget estimates were necessary to carry out programs in reactor development and in applications of isotopes and nuclear explosives for civilian uses. The committee stressed its belief that the civilian program in these fields should now be supported by the government, although it recognized "that eventually industry should provide the major fund support." On 18 August the President signed an appropriations bill for $2.65 billion.

Also in Senate-House conference is a supplemental appropriations bill that will provide funds for the operation of the National Aeronautics and Space Administration. The House, which originates all money bills, had cut $68 million from the Administration's request for NASA. The Senate, acting on the same bill, restored the cut after T. Keith Glennan, administrator of NASA, said that any sizable reduction in the $530 million budget request would permit the U.S.S.R. to take undisputed leadership in the space technology field. Glennan also suggested that the country's man-in-space program—Project Mercury, would be retarded. At this writing no action has been taken on the reconciliation of the two bills.

Other Bills

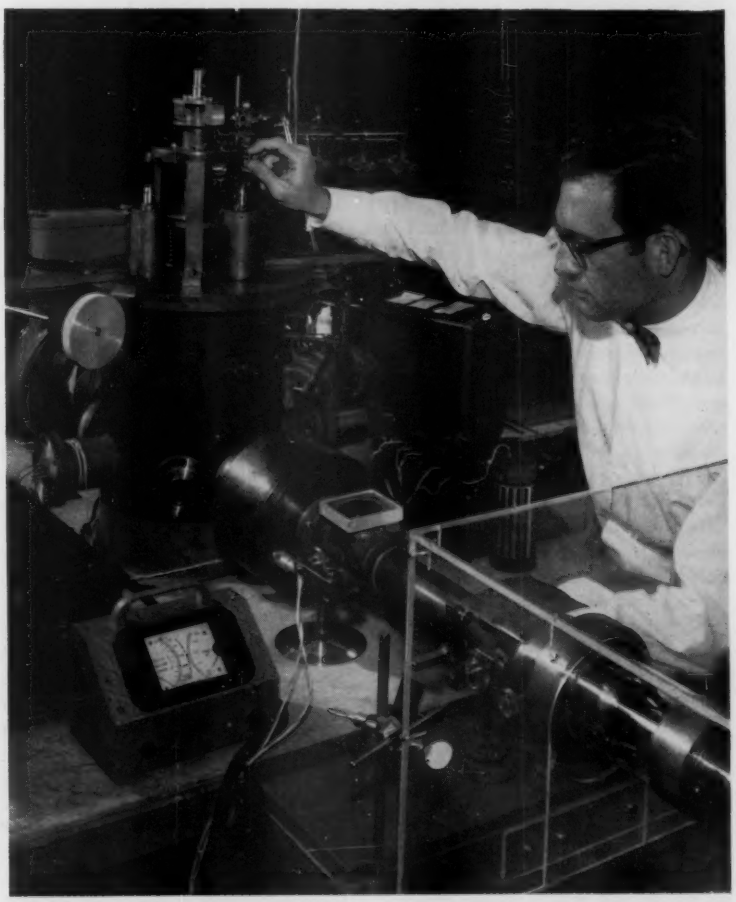
Other bills of interest to scientists, not related to appropriations, are also before Congress. Two conservation bills, one dealing with clean streams and the other with wilderness areas, may be acted on by the whole Congress before adjournment. Action on the wilderness bill, which would establish a national wilderness preservation system, was delayed by the Senate Committee on Interior and Insular affairs on 14 August. In the view of some observers, the bill, if reported out favorably, has a fair chance of being passed this session. The clean streams bill, sponsored by Representative John Blatnik (Democrat-Farmer-Labor-Minn.) has been passed by the House and is now pending before the Senate. The measure would amend the Federal Water Pollution Control Act to increase the authorization of construction grants for sewage treatment works at $100,000 a year over a 10-year period. Present annual allocations, which are generally held to be inadequate, total $45,000.

NBS Studies Trapped Radicals

To provide basic data on the properties and behavior of stabilized free radicals, the National Bureau of Standards, under the sponsorship of the Department of Defense, has made an extensive spectroscopic study of highly reactive atoms and excited molecules trapped in solids at low temperature. A number of methods are available for producing such electronically excited molecules and atoms in solids at low temperatures.

The Bureau employed primarily two techniques in order to present a consistent over-all picture of observations. In one aspect of the work, M. Peyron, guest research worker from the University of Lyon, collaborated with H. P. Broida and H. W. Brown of the NBS Free Radicals Section in investigating reactive fragments condensed from nitrogen gas passed through an electric discharge. In the other phase of the study, carried out by E. Hörl of the Bureau's electron physics laboratory, nitrogen atoms were produced by the electron bombardment of nitrogen condensed on a cold surface, as shown in the illustration. Analogous studies with oxygen were made by L. Schoen and H. P. Broida.

Free radicals have been known to exist for about 30 years but only lately have they become the object of widespread interest. The National Bureau of Standards is now engaged in a 3-year program of free radical studies.



Apparatus used in National Bureau of Standards studies of trapped radicals produced by the electron bombardment of nitrogen condensates. Electrons from the electron gun in foreground at right impinge on the helium-cooled, nitrogen-coated target inside the cylindrical cryostat. The spectroscope used in studying the emitted light is not shown.

Federal Radiation Council Established; States to Get New Responsibilities

The President has issued an executive order that establishes a Federal Radiation Council. This action centralizes the responsibility for providing general standards and guidance to executive agencies for their use in developing operating rules and regulations for radiological health protection. Members of the new council are the heads of the agencies most significantly involved with radiation: the Secretary of Health, Education and Welfare, the Chairman of the Atomic Energy Commission, the Secretary of Defense, and the Secretary of Commerce. The President's Special Assistant for Science and Technology will participate as an adviser in the discussions of the council.

In developing its advice to the President, the council will consult with appropriate agencies, such as the Departments of Labor and Agriculture, and will solicit the views of scientific bodies —for example, the National Committee on Radiation Protection and Measurement and various committees of the National Academy of Sciences. The Federal Radiation Council will also take steps designed to further the interagency coordination of measures for protection against radiation, and to that end will consult with all federal agencies that have radiological health responsibilities.

Establishment of the council follows recommendations made to the President by the Secretary of Health, Education, and Welfare, the Chairman of the Atomic Energy Commission, the Special Assistant to the President for Science and Technology, and the Director of the Bureau of the Budget.

In addition, the President has approved a series of recommendations to be carried out upon enactment of proposed legislation endorsed by the Administration (S.1987 and H.R.7214) under which certain regulatory responsibilities of the Atomic Energy Commission will be transferred to the states by agreement with the commission. The recommendations are that:

1) The AEC have the principal federal responsibility for preparing the states for the proposed transfer of certain of its regulatory responsibilities.

2) The training programs necessary for such transfer be financed and planned by the commission, and that in order to make maximum use of existing facilities and competence, such programs be conducted under cooperative arrangements between the AEC and the Department of Health, Education, and Welfare.

3) At the termination of this special training program, any training of state personnel be conducted within the continuing programs of the Department of Health, Education, and Welfare and other federal agencies.

4) The Department of Health, Education, and Welfare continue as the federal focal point for guidance and assistance to the states with respect to contamination by and biological effects from radiation sources not now under control of the AEC.

Overseas Research Council Announced in Britain

Great Britain has announced the formation of the Overseas Research Council, a coordinating agency for scientists both within and without the Commonwealth. The new organization, probably to be known as "Oresco," has been established primarily because of the pace of political change in Britain's overseas territories, according to the *Manchester Guardian*. The announcement of the project was overshadowed by the almost simultaneous announcement of Britain's space research program. The *Guardian* observes that the result of this timing was to "muffle the start of a project that may well prove more important to this country in the long run than the launching of a few satellites."

The Overseas Research Council was established to advise the Privy Council's newly formed Committee on Overseas Research, which consists of the Lord President of the Privy Council and the Secretaries of State for Commonwealth Relations, the Colonies, and Foreign Affairs. In a statement about the new council, Lord Hailsham, the Lord President, said:

"The United Kingdom Research Councils—Department of Scientific and Industrial Research, Medical Research Council, and Agricultural Research Council—are willing to provide advice and assistance on research matters falling within their respective fields to any Commonwealth country desiring such assistance, wherever this can be done within the funds available. The Overseas Research Council will provide for the co-ordination of this assistance, and for the formulation of general U.K. policy in this field. The Council will provide a central point to which Commonwealth Governments and research institutions can refer for advice and information, and will advise generally on U.K. co-operation in scientific research overseas.

"There are no geographical restrictions in the Council's terms of reference. Matters concerning scientific development in Colonial territories, in Commonwealth countries and in countries outside the Commonwealth, can equally be referred to it. Moreover, in promoting such development, the Council can look to possible collaboration between this country and other Commonwealth countries, countries outside the Commonwealth, such as the U.S.A., and international agencies (such as U.N. agencies and the charitable foundations)."

The members of the Council are: R. S. Aiken (chairman), vice chancellor, University of Birmingham, and chairman, Committee of Vice-Chancellors and Principals; Sir Jock Campbell, chairman, Booker Bros., McConnell & Company, Ltd., and chairman, Imperial College of Tropical Agriculture; Sir Charles Dodds, Courtauld professor of biochemistry, University of London, and director of Courtauld Institute of Biochemistry, Middlesex Hospital; Sir Harold Himsworth, secretary, Medical Research Council; Sir Joseph Hutchinson, professor of agriculture, Cambridge University; R. Lewthwaite, director of colonial medical research, Colonial Office; John McMichael, professor of medicine, University of London, and director, department of medicine, Postgraduate Medical School of London; Sir Harry Melville, secretary, Department of Scientific and Industrial Research; G. W. Nye, agricultural adviser to the Secretary of State for the Colonies; Sir Arnold Plant, Sir Ernest Cassel professor of commerce in the University of London at London School of Economics; Sir William Slater, secretary, Agricultural Research Council; H. G. Thornton, foreign secretary, Royal Society, and former head of the department of soil microbiology, Rothamsted; and Sir Solly Zuckerman, deputy chairman, Advisory Council on Scientific Policy.

Canadian Oceanographic Institute

Canada, which possesses the longest coastline of any country in the world, is establishing a $3-million oceanographic institute on the east coast, in Bedford Basin near Halifax. The new institute, which will be under the Department of

Mines and Technical Surveys, will take 5 years to complete. When in operation, it will have a staff of some 300 oceanographers, hydrographers, submarine geologists, and other scientific personnel, plus supporting staff, and an operating fleet of ten oceanographic vessels. A multi-million-dollar ship-building program is already under way to provide the fleet of vessels; it is expected that the first of these, the $7-million *C.G.S. Hudson*, will be commissioned in 1961.

The establishment of the institute, which is to be known as the Bedford Institute of Oceanography, was announced on 5 August by Paul Comtois, Minister of Mines and Technical Surveys. He reported that the Bedford facility will study the physical characteristics of the waters and underlying sea bed of Canada's Atlantic and subarctic coasts. The resultant data are needed for anti-submarine defenses and to ascertain the resource potential of the continental shelf in these regions.

The new organization will also permit the expansion of the Atlantic and subarctic sections of the Canadian Hydrographic Service. These sections will be moved from Ottawa to Bedford Basin, a reorganization that will greatly facilitate hydrographic operations in eastern and northern areas, where most of the coastline is uncharted. In addition, the institute will house the regional office of the Geological Survey of Canada.

Comtois pointed out that the project will mean the building up, near Dartmouth, of a strong center of marine science. There will be liaison with the Fisheries Research Board, the Atlantic Oceanographic Group (to be housed in the new institute), and with Dalhousie University, which is setting up—with the help of the National Research Council grant—an Institute of Oceanography for the training of scientists, many of whom will be employed by the new federal institute. The center will also be the headquarters of the polar group of oceanographers, hydrographers, geologists, and other scientists working in the icebound sections of the remote arctic. They will carry out a broad program of oceanographic research on the rim of the Arctic Basin.

Canada possesses little knowledge of the oceans which surround it. Except for a specialized program in oceanography by the Fishieries Research Board, conducted over the years, oceanography in Canada has been a neglected science, mainly because of the size and great expense of the job to be done.

Project Teepee Monitors Missile Shots and Upper Air Explosions

An electronic surveillance system, capable of detecting missile firings and explosions of nuclear weapons in the upper atmosphere, has been monitoring Soviet space activities for the past several months. The system, which is operated from bases within the continental U.S., can pick up such firings regardless of their point of origin. Missile launchings in this country and Russia have been successfully monitored by the new system, as well as some of the atomic weapon tests that were conducted in the Pacific before the suspension last October.

The Teepee system was developed by members of the Office of Naval Research working under W. J. Thaler. It is able to detect targets beyond the horizon by bouncing signals in a zig-zag pattern between the earth and the ionosphere. At each point of bounce there is some reflection of the signal back to its point of origin. This return, called backscatter, has certain characteristics as it appears on receiving screens at the transmitting-receiving station. If the radio signals encounter large volumes of hot gases, such as those created by rocket firings or atomic weapon tests in the upper atmosphere, the characteristics of the backscatter are significantly different. By analysis of these characteristics, operators at Teepee stations can identify the source of the gases. The new system is said be capable of distinguishing between large and small missiles and between successful and unsuccessful firings. It is also said to be able to discriminate between natural phenomena, such as lightning and aurora, and man-made disturbances.

The Teepee system, which Thaler believes can be much improved, complements other missile and test detection systems which are now in use, or planned, such as the Ballistic Missile Early Warning System, certain powerful radar sets in Turkey which can scan much of the Soviet Union, and a proposed satellite system which would use infrared sensors to detect launchings and blasts.

These systems, supported by the new Teepee technique, which is said to be able to detect more than 95 percent of all atmospheric weapons tests and rocket launchings, are believed by many observers to give the United States fairly thorough and current knowledge of Soviet progress in missile and weapons development.

Mueller Is Commerce Secretary

Frederick H. Mueller, who served as acting Secretary of Commerce following the resignation of Lewis L. Strauss after his long and unsuccessful confirmation fight, was given the oath of office for the top Commerce position on 10 August. He had been nominated to the cabinet post 21 July and was confirmed for the job in early August.

Mueller, who has been with the department since November 1955, has held the posts of Assistant Secretary for Domestic Affairs and Under Secretary. He was born in Grand Rapids, Mich., and was educated in the state, receiving his bachelor of science degree from Michigan State University in 1914.

As Secretary of Commerce, Mueller will have responsibility for many units of the department which carry on scientific work. Among these are the National Bureau of Standards, the Weather Bureau, and the U.S. Coast and Geodetic Survey.

Dismissed Arkansas Professors Receive Aid from Fellow Teachers

Four University of Arkansas professors, dismissed for refusing to file affidavits under a new Arkansas law, will receive the full support of the American Association of University Professors in finding posts elsewhere. William P. Fidler, AAUP General Secretary, has also announced that the professors will get significant financial assistance from the association's Academic Freedom Fund.

Act 10 of the Arkansas Statutes, passed at a special 1958 session, requires all publicly employed teachers to list the organizations to which they have belonged or to which they have contributed during the past 5 years. The act is generally regarded as an anti-NAACP measure, but the language covers churches, political parties, social clubs, and professional societies. A test case has been started, but a final legal verdict, especially if it involves constitutional questions pertaining to civil liberties, may be delayed.

Max Carr, Frederick G. Friedmann, John L. McKenney, and Thelma W. Taylor (whose fields are philosophy and music) base their refusal on principle. One of the teachers quotes Jefferson: "to suffer the civil magistrate to intrude his powers into the field of opinion and to restrain the profession or propagation of principles, on the supposition of their

ill tendency, is a dangerous fallacy, which at once destroys all religious liberty. . . ." The teacher then goes on to speak for himself: "I have taught the writings of Thomas Jefferson year after year in my courses. . . . I cannot refuse to heed his words in my own life."

The AAUP has sent out 1000 letters urging its 42,000 members to explore job possibilities for the four professors. In making its action a matter of public record, the association hopes to enlist the interest of the millions of Americans who, as alumni of colleges, respect the academic profession.

As to interim financial aid, Fidler stated: "The resources of our recently established Academic Freedom Fund are limited, and we regret that we cannot give financial assistance to every college teacher who loses his post for improper reasons. However, the teachers in Arkansas are victims of a law which strikes at the heart of academic freedom by restraining them from joining organizations of their choice. The AAUP stands ready to give every assistance within its power to the courageous teachers who sacrificed their positions rather than submit to Act 10."

Soviet Technical Journals

A listing of abstracted Russian technical journals currently available by subscription from the Office of Technical Services, U.S. Department of Commerce, has just been published. The listing shows some 100 Soviet technical periodicals abstracted regularly by U.S. government agencies and released to the public through OTS as part of its program of collecting and disseminating translated technical literature.

The periodicals cover such fields of research as areonautics, astronomy and mathematics, chemistry and chemical engineering, civil and electrical engineering, fuel and power, geography and geology, mechanical engineering, mining and metallurgy, physics, and general science and technology. Included are the physics, chemistry, and biology series of *Referativnyy Zhurnal,* the U.S.S.R.'s central abstracting journal.

The new listing supersedes one published in August 1958 at the outset of the OTS translation program. Certain periodicals have been added to the collection during the past year, and others have been dropped because of limited usefulness or for other reasons.

The listing, *English Abstracts of Russian Technical Journals,* is available without charge from OTS, U.S. Department of Commerce, Washington 25, D.C.

Britain Uses New Reactor Crane

An unusual two-shafted crane, known as "Goliath," has been designed especially for nuclear reactor construction tasks. It is in use at Hinkley Point, Somerset, England, where the largest atomic power station so far announced by any country is being built.

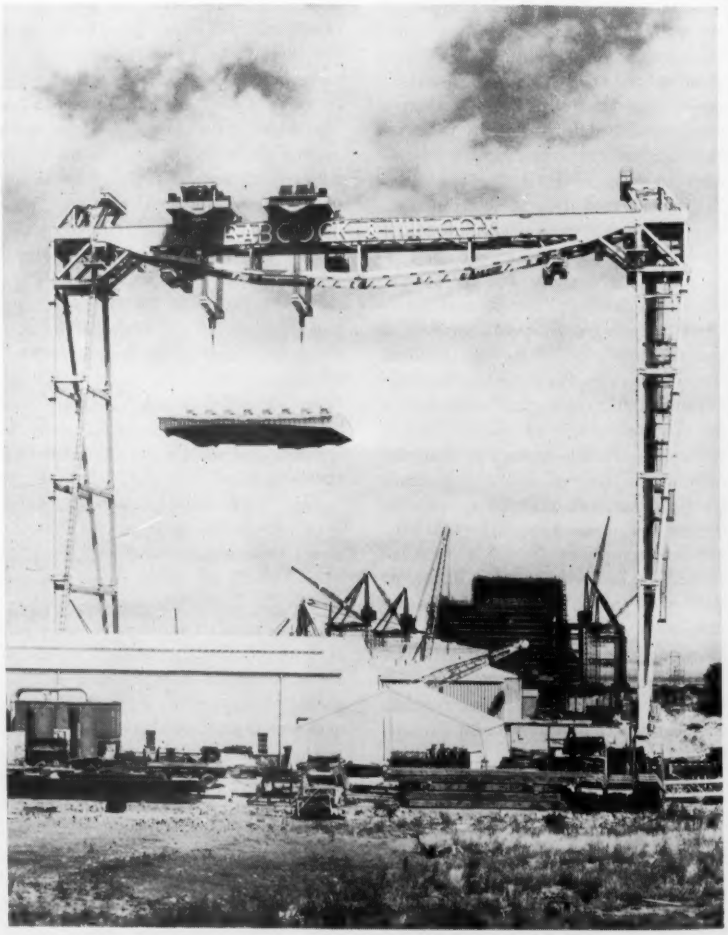
The new facility, which will cost $128 million, is located on a 140-acre site. It is expected to be in operation in 1962 with an output of 500 milliwatts.

The picture shows Goliath raising a temporary weather roof for one of Hinkley Point's two reactors. The new lifting equipment has a capacity of 400 long tons.

AEC Gives Views on Radioactive Waste Disposal at Sea

Following are excerpts from testimony by A. R. Luedecke, general manager of the Atomic Energy Commission, on the National Academy of Sciences–National Research Council report titled "Radioactive Waste Disposal Into Atlantic and Gulf Coastal Waters." The testimony was given on 29 July before the Special Subcommittee on Radiation of the Joint Congressional Committee on Atomic Energy.

The [National Academy of Sciences–National Research Council] report was requested and sponsored by the Bureau of Commercial Fisheries, Office of Naval Research, and the Atomic Energy Commission. The nature of the request was to examine the feasibility of disposing of the low-level wastes closer to shore than the 1000 fathom disposal sites used by AEC. Feasibility was considered primar-



A special crane for reactor construction, in use in England.

ily from the point of view of safety. The study group considered inshore areas as safe for disposing of solid or packaged, low-level wastes up to 250 curies per year of the most biologically significant isotope without causing any adverse effect on man, provided proper precautions are taken in assessing and utilizing the sea disposal sites.

Publication of the NAS–NRC report appears to have led to a misconception that the [Atomic Energy] Commission would approve the suggested sites for immediate use. There is no urgent requirement for inshore disposal sites. The Commission has no intention of designating any such site in the future without first thoroughly investigating, with the assistance of other interested federal and state agencies, the physical and biological conditions of the area, as recommended by the NAS–NRC committee.

If a site were definitely established as suitable for use as a disposal ground without endangering the health and safety, the Commission would not license the commercial use of the site without providing an opportunity for a public hearing on the matter, as required by its rules of practice.

In anticipation of a possible need for a coastal disposal site in the New England area where there is a relatively heavy concentration of industrial, medical, university and other users of radioisotopes, the Commission is making arrangements to conduct field investigations of four Atlantic Ocean areas off the New England coast to determine if a specific site could be designated, when needed, for the safe disposal of small quantities of low level radioactive wastes and to establish the conditions under which such wastes could be deposited at the site. The studies, to begin in September, will be conducted with the assistance of the Coast and Geodetic Survey, the Public Health Service and scientists from the University of Connecticut. The AEC is financing all of this work.

The Commission does not presently contemplate investigating any other suggested Atlantic coastal areas because a need for their use is not foreseen in the near future. The Commission tentatively plans to investigate areas in the Gulf of Mexico sometime next year. None of the suggested Gulf locations has been designated for this study as yet.

Sites Eliminated

After consultation with representatives of the NAS–NRC Committee, local representatives of the United States Fish and Wildlife Service and representatives of the Massachusetts Departments of Public Health and Marine Resources, the AEC has eliminated from its consideration two of the inshore disposal sites in the New England area suggested in the NAS–NRC report because use of these areas might interfere with sport and commercial fishing activities. These sites are: A rocky ledge, known as "Browns Ledge" 10 miles from Sakonnet, Rhode Island, at 41°19′7N and 71°063′W, and a two-mile-diameter site in which unexploded depth charges have been dumped, located 10 miles from Point Judith, Rhode Island, at 41°14′N and 71°25′W.

Two of the four sites to be investigated were specifically suggested by the NAS–NRC Committee in its report. One of these sites is an area two miles in diameter, located in Massachusetts Bay at 42°25.5′N and 70°35′W, which has been used by the Crossroads Marine Disposal Corporation of Boston as a disposal ground for small quantities of low level wates. The company's license has been amended to require that as of August 15, 1959, it carry out its disposal operations in deep waters (1000 fathoms) off the continental shelf, at two locations—one 150 miles southeast of Sandy Hook, New Jersey and the other 200 miles off Cape Cod. The other site is an explosives dumping area, 10 miles by 10 miles, located 45 miles from Sakonnet Point, Rhode Island, at 40°45′N and 70°52′W.

The other two sites were not identified in the NAS–NRC report but were suggested by the NAS–NRC Committee, local representatives of the United States Fish and Wildlife Service and representatives of the Massachusetts Departments of Health and Marine Resources for further investigation. They are near sites included in the NAS–NRC report. One site is a 10-mile by 10-mile area known as No Man's Land. This is an area already restricted and used as a Navy gunnery range. It is approximately 12 miles south of Martha's Vineyard at 41°15′N and 70°43′W. The other is an area reported to be devoid of biological life located approximately 15 miles east of South Wellfleet, Massachusetts, at 42°05′N and 69°46′W.

The Coast and Geodetic Survey will take samples and make measurements to evaluate the dispersing effect of tides and currents and the uptake of radioactivity by clays and silts and by biological systems. The biological sample gathering will be carried out in collaboration with Dr. John S. Rankin, marine biologist of the University of Connecticut. The Water Supply and Water Pollution Control Group of the Public Health Service Robert A. Taft Sanitary Engineering Center at Cincinnati, Ohio, will make measurements of radioactivity in the biological and sediment samples to determine background radiation conditions.

After the field data have been gathered and analyzed the Commission will convene a group of marine scientists to evaluate the results. In the course of the evaluation other Federal and State agencies having an interest in the matter will be consulted.

If one of these inshore disposal sites is approved, periodic monitoring of the site would be carried on in order continuously to assure that the capacity of the site to receive these radioactive materials safely is not exceeded.

Basic Considerations

In evaluating or establishing any waste disposal system, three basic considerations are involved as follows:

1. The specific nature and quantity of the radioactive waste to be disposed of.

2. The characteristics of the receiving environment.

3. Basic radiation protection standards established by the Commission in its regulation, Standards for Protection Against Radiation (10 CFR 20).

The radiation protection standards established by the Commission are based on the best available biological and medical information and on recommendations of the National Committee on Radiation Protection and the International Committee on Radiation Protection. The recommendations of the two committees have been agreed to by various national and international organizations.

The radioactive material involved in AEC sea disposal operations off both the Atlantic and Pacific Coasts is of a relatively low or intermediate level compared with highly radioactive wastes produced at AEC production sites such as Hanford or the National Reactor Testing Station. The wastes disposed at sea contain quantities of radioactivity normally associated with research and development activities rather than production or chemical reprocessing. For example, in terms of radioactivity concentration, the relatively small quantity of liquid wastes finally disposed at sea (after solidification) are less than a curie and generally in the thousandth or millionth of a curie per gallon range, whereas the liquid high-level waste re-

sulting from chemical processing operations at Idaho might have concentrations in the hundreds or thousands of curies per gallon. Thus we have a factor of difference in concentration of the order of tens or hundreds of millions. Also, the total number of gallons (i.e., total quantity of radioactivity) evolving from the two situations is vastly different. As an example, in 1957 the AEC disposed off both coasts of the United States at designated locations 686 55-gallon drums of solidified laboratory waste liquids (this volume includes concrete and other solidifying agents). On the other hand there are about 65 million gallons of high-level wastes in storage at Hanford, Savannah River, and Idaho. We do not propose to dispose of these at sea, even though some oceanographers may indicate that a "dilute and disperse" waste disposal approach may be theoretically possible.

The wastes considered for sea disposal originate in various AEC research and development operations and in research laboratories of hospitals, universities, industrial firms and other places where radioactive isotopes are used for various purposes. The radioactive waste itself usually is in the form of contamination on equipment such as test tubes, bottles, rubber gloves, blotting paper, and rubber tubing. This trash is packaged within concrete in 55-gallon drums or in preformed, reinforced concrete boxes before disposal.

Although experience in other countries has demonstrated the safety and practicality of disposal of bulk radioactive liquids at sea, all radioactive wastes disposed of by the AEC and licensees off both the Pacific and Atlantic Coasts of the United States have been in the solid or packaged form, with two minor exceptions. These exceptions involve millicurie quantities. Most of the waste has been contained in 18-gauge, 55-gallon drums with concrete liners and concrete tops and bottoms for weighing and shielding purposes.

New Legislation Opposed

H.R. 8187 is a bill to impose certain restrictions on the disposal of radioactive material in the Gulf of Mexico. It would prohibit the disposal of any radioactive material in the Gulf of Mexico at a point a) less than 200 miles from the shoreline of any State of the United States; b) where the water is less than 1000 fathoms deep or c) where the waters are used customarily for commercial sports or fishing.

In addition, it would prohibit the disposal of any radioactive material into the Gulf of Mexico unless the material is in a container of "such character and strength that it will remain intact indefinitely, regardless of the depth of the water in which it is deposited." Moreover, if the shipment of radioactive material originated or was assembled in a State bordering on the Gulf of Mexico, disposal in the Gulf would be prohibited unless permission to dispose of the material had been obtained from the State or an authorized official or agency of the State.

Since receiving the request of the Joint Committee for comments on H.R. 8187, we have not had an opportunity to prepare detailed comments on the bill. We should like to say, however, that the Commission would not look favorably on the bill.

The disposal of radioactive material into the seas (including the Gulf of Mexico), involves detailed considerations of many technical factors. These include a) the nature and characteristics of the radioactive materials; b) the oceanographic features of the site proposed for disposal, including the ocean currents and the biological characteristics; c) the nature of the packaging methods; d) other technical factors that may be involved in the particular disposal activity.

We believe that such questions can more appropriately be resolved by quasi-judicial and quasi-legislative procedures, subject to the Administrative Procedures Act, as carried out by the Atomic Energy Commission and other Federal administrative agencies. We believe that it would be undesirable to establish by legislation specific prohibitions which do not take into account the many varying, technical and scientific considerations involved in this complex subject.

News Briefs

Scientists from 18 countries have begun a 2-month training program at Cornell University on uses of atomic energy in agricultural research. The program is sponsored by the U.N. Food and Agriculture Organization and the International Atomic Energy Agency.

* * *

Columbia University and the California Institute of Technology are about to begin construction of a "lunar seismograph." Under contracts from the National Aeronautics and Space Administration, seismological experts from the two institutions plan to develop a "moonquake" detection system to be established on the moon. The seismograph's signals would be monitored at stations on earth. At Columbia the work will be carried out by the Lamont Geological Observatory, Palisades, N.Y.; at C.I.T., by the Seismological Laboratory.

* * *

The August issue of the *Microchemical Journal* introduced a new feature in presenting its first "Annual Progress Number." Published as the third of a year's four issues, each annual progress number will cover advances in microchemistry made during the year. The *Microchemical Journal* was launched in April 1957 under the auspices of the Metropolitan Microchemical Society, New York. It is published by Interscience Publishers.

* * *

An agreement to establish the first joint nuclear research center of the European Atomic Energy Community has been signed by the Euratom Commission and the Italian Government. The center, to be located in Ispra on the shores of Lake Maggiore in northern Italy, will be manned by 1200 scientists from the six Euratom countries by the end of 1962.

* * *

Progress in Atomic Energy, volume 1 of the proceedings of the second United Nations Conference on the Peaceful Uses of Atomic Energy, is now available. The volume, which contains 525 pages, 218 illustrations, and 162 charts and graphs, can be ordered through any bookstore or directly from the United Nations, Sales and Circulation Section, New York, N.Y. (price, $12.50).

* * *

A new major, Mathematics for Teachers, will be offered this fall by the mathematics department at the University of California, Berkeley, to provide training for future high-school teachers of mathematics. Upon completion of the major, students may enter the internship program of the department of education, which consists mainly of a year of intern teaching at full salary, or they may take a fifth year of work in education and earn a teaching credential in the usual way.

Each year a leading professor of mathematics from a California state college will be appointed visiting professor at the Berkeley campus to assist in directing the program. For 1959–1960, the visiting professor will be Roy Dubisch, chairman of the mathematics department at Fresno State College.

The new program is supported in part by a $50,000 grant from the National Science Foundation.

Scientists in the News

WILLIAM R. AMBERSON, professor of physiology and head of the department at the University of Maryland (Baltimore), retired on 31 July. He was a valued member of the AAAS editorial board from January 1951 through June 1955. At present Amberson is at the Marine Biological Laboratory in Woods Hole, Mass., where he expects to remain for at least 2 years to conduct research under the auspices of the National Institute of Arthritis and Metabolic Diseases, National Institutes of Health.

In collaboration with ADELIA BAUER, until recently of the department of physiology at Vassar College, he will continue his study of the fibrous muscle proteins. The two investigators are particularly concerned with their newly discovered delta protein, a fibrous molecule that can unite with myosin and cause the partial dissociation of actomyosin.

J. HAMILTON CRAWFORD, clinical professor of medicine at the State University of New York Downstate Medical Center, Brooklyn, has retired and has been named clinical professor emeritus of medicine. A graduate of the University of Edinburgh, Scotland, Crawford served as surgeon in the Royal Navy from 1915 to 1920 and taught at Edinburgh University from 1921 to 1923. He came to this country in 1923 as assistant in cardiology at the Rockefeller Institute for Medical Research, New York. He became associate in medicine at the Long Island College of Medicine in 1928. Crawford will continue as attending physician at the Long Island College Hospital, Brooklyn.

GWILYM A. PRICE, chairman of the board of the Westinghouse Electric Corporation, will receive the John Fritz Medal at the annual meeting of the American Society of Mechanical Engineers, which will be held in Atlantic City in early December.

LEONARD M. LIBBER, physiologist at the Air Crew Equipment Laboratory, Philadelphia, has been appointed head of the Physiology Branch of the Biological Sciences Division of the Office of Naval Research, Washington, D.C. He succeeds Captain T. K. RUEBUSH, who has been placed in charge of research in the field of parasitology at the Naval Medical Research Unit No. 3 in Cairo, Egypt.

ALVIN W. GOULDNER, professor of sociology at the University of Illinois, has been appointed professor of sociology and chairman of the department of sociology-anthropology at Washington University's College of Liberal Arts. He succeeds NICHOLAS J. DEMERATH, who will continue as professor of sociology and director of the Social Science Institute.

HERNDON G. DOWLING, associate professor of zoology at the University of Arkansas, has been appointed associate curator of reptiles at the Bronx Zoo, New York. He succeeds JAMES A. OLIVER, who has been appointed director of the American Museum of Natural History.

The following American scientists have been elected to membership in the Polish Academy of Sciences:

ROGER ADAMS, professor of chemistry at the University of Illinois; WILLIAM PRAGER, professor of applied mechanics at Brown University, Providence, R.I.; and ANTONI ZYGMUND, mathematician at the University of Chicago.

T. O. HALL, president of the General Geophysical Company, Houston, Tex., has been named president of the Society of Exploration Geophysicists.

E. JEFFERSON BROWDER, chairman of the department of neurosurgery at the State University of New York Downstate Medical Center, Brooklyn, has retired and has been named professor emeritus of neurosurgery. Browder, who is internationally known for his work in the surgical treatment of parkinsonism and head trauma, has been on the faculty for 34 years. He has been chairman of the department of neurosurgery since 1952 and for the four preceding years he served as chairman of the department of surgery. He was graduated from the Johns Hopkins Medical School in 1920 and completed his internship and residency training at the Long Island College Hospital in Brooklyn in 1927. Browder will continue to practice in Brooklyn and to carry on special research in the surgical treatment of patients with brain tumors.

HAROLD ORLANS, formerly head of the Foreign Studies Section, National Science Foundation, will be on leave of absence for a year to serve as director of studies for the 1960 White House Conference on Children and Youth.

THOMAS L. ALLEN of the University of California, Davis, has been appointed visiting research scholar in the department of chemistry at Indiana University for the academic year 1959–60.

HUGH J. MISER, formerly deputy assistant for operations analysis at U.S. Air Force Headquarters, has been appointed head of the Operational Sciences Laboratory of the Research Triangle Institute, Durham, N.C.

Recent Deaths

NOAH E. DORSEY, Towson Md.; 86; retired physicist of the National Bureau of Standards, Washington, D.C., where he had been employed for more than 40 years; author of *Properties of the Ordinary Water Substance*; 6 July.

JOHN G. KIRKWOOD, New Haven, Conn.; 52; chairman of the department of chemistry and director of the division of sciences at Yale University; 9 Aug.

LOUISE PEARCE, New York, N.Y.; 74; biochemist, retired associate member of the Rockefeller Institute, and former president of the Women's Medical College of Philadelphia; codeveloper of the first cure for African sleeping sickness; 9 Aug.

JAMES D. SCHOFIELD, Philadelphia, Pa.; 80; retired clinical professor of proctology at Hahnemann Medical College and chief of the proctological staff of Hahnemann Hospital; 9 Aug.

ROBERT K. SPEER, New York, N.Y.; 61; professor of education and former chairman of the department of elementary education at New York University; 9 Aug.

MILTON A. TREUHAFT, Livingston, N.J.; 43; research engineer for the Microwave Research Institute, New York; taught mathematics at Brooklyn Polytechnic Institute; 25 July.

RUSSELL H. VARIAN, Juneau, Alaska; 61; physicist and a founder and board chairman of Varian Associates; coinventor of the klystron tube, an electronics instrument vital to radar; holder of more than 100 patents; 29 July.

OSCAR VOGT, Freiburg, Germany; 90; director of the Institute for Brain Research in Neustadt, Germany; formerly director of the Kaiser Wilhelm Institute for Brain Research in Berlin; 3 Aug.

RAY R. WORTHY, Saltville, Va.; 68; engineer and former president of the Olin Mathieson Chemical Corporation; 26 July.

Book Reviews

A History of Technology. vol. 4, *The Industrial Revolution, c 1750 to c 1850.* xxxiii + 728 pp. Illus. + plates. vol. 5, *The Late Nineteenth Century, c 1850 to c 1900.* xxxviii + 888 pp. Illus. + plates. Charles Singer, E. J. Holmyard, A. R. Hall, Trevor I. Williams, Eds. Oxford University Press, New York, 1958. $26.90 each.

Readers of the first three volumes of this immensely successful cooperative venture among scholars will not be disappointed in these, the concluding, volumes. Even though as the editors point out, limitations of space increased tremendously the writers' and the editors' problems in dealing with the more recent technology, these volumes exhibit the same outstanding scholarship and competent judgment that we have come to know in the earlier volumes. It is inevitable that the history of "modern" technology—from approximately the beginning of the 19th century to the present—proliferating as it does in conelike fashion, should present especially difficult problems for the historian. The data are infinitely more numerous than for earlier periods, and thus critical problems of selection are presented, if the data cited are to be representative. Moreover, for a variety of reasons, the history of modern technology has been studied less by professional historians of technology than the earlier history. In short, the "rapidly increasing complexity of the subject" forced the editors to be more and more selective in their choice of subject matter. And yet, despite this apparent disadvantage, the result is a cogent and convincing documentation of the technological revolution which led, without respite, to present-day Western civilization.

The period covered in volume 4, roughly 1750 to 1850, is generally identified as that of the Industrial Revolution. That the events of an entire century should have come to be known as a "revolution," rather than merely an "evolution," may seem paradoxical, and yet the true revolutionary character of these events is clearly discernible in this study. The events of this century completely revolutionized man's attitude toward his environment and, coincidentally, enabled him to utilize as never before, almost all natural resources. Developments in technology during this century literally redefined Western civilization. The shock wave of new inventions and technical processes struck first in England, then on the Continent and in North America. With it came radical economic, agricultural, and mercantile changes, which led eventually to the notion of the modern industrial state.

The editors have treated this period in terms of six broad fields of classification: (i) "Primary production" (foods, metals, and coal); (ii) "Forms of energy" (the production of power in general, the early development of the steam engine and the water mill); (iii) "Manufacture" (the chemical industry, gas production, textiles, ceramics, glass, precision machines, and machine tools); (iv) "Static engineering" (civil and sanitary engineering); (v) "Communications" (roads, canals, shipbuilding, cartography, dredging, and early telegraphy); (vi) "Scientific basis of technology."

This final chapter will be especially interesting to historians of science, for the period of the Industrial Revolution marks the beginning of the permeation of technology by science—"the beginnings of the change from craft mystery to science as a basis for technology." The deliberate shift from secrecy to scientific openness was perhaps the most important single step in the development of modern technology. The story of this movement is exciting as well as important. Institutions such as the Royal Society of London and the much younger British Association for the Advancement of Science played a role no less vital than that of the great individual scientists and engineers, among them Joseph Black, Antoine Lavoisier, Michael Faraday, Lord Kelvin, Count Rumford, Denis Papin, Thomas Newcomen, and James Watt, to mention but a few. In France, one of the greatest systematizing influences on contemporary technology was the famous *Dictionnaire raisonné des sciences des arts et des métiers,* edited by Denis Diderot and Jean d'Alembert. Works such as this helped to generate the surging enthusiasm of the late 18th and early 19th centuries over the power of applied science as a means of bettering man's lot.

In volume 5 we are given a continuation of this *History,* through the second half of the 19th century. The broad organizational scheme is modified slightly. Part 1 treats of "Primary production"; under this heading again come foods and metals but, significantly, a chapter on the production and uses of petroleum has been added. Part 2 deals with "Prime movers": the stationary steam engine, the marine steam engine, and internal combustion engines. It may surprise readers to learn that the idea of the internal-combustion engine antedates that of the piston steam engine. As early as the latter part of the 17th century Christian Huygens experimented with the gunpowder engine. In part 3 we read of the "Rise of the electrical industry," and of the generation, distribution, and utilization of electricity. Part 4 treats of the chemical industry. Part 5 deals with transportation, including early aeronautics. Part 6 is concerned with civil engineering. Part 7 discusses all aspects of manufacturing, including newly developed areas of technology such as photography. The eighth and final part of volume 5 is entitled, "The threshold of the twentieth century." Here are treated, in broad but significant fashion, the problems of education, industrial organization, and the social consequences of technology. These aspects of the modern industrial state will perhaps be more familiar than some of the others to the average reader of this *History,* but this presentation is both authoritative and fresh.

Thus, this work brings the history of technology down to approximately the beginning of our own century. This terminal date is both convenient and symbolic. From the beginning, when this work was in the planning stage, the editors realized that it would be impractical to carry it on to the present time, despite the obvious attractiveness of such an objective. The amount of space needed to describe the technological progress of the past half-century alone would have made the project unfeasible. In addition, technology today is relatively so complex and so technical that adequate descriptions would, of neces-

sity, have involved the use of much more technical language than was required in the present five volumes. Moreover, the beginning of the 20th century signalized a new technological revolution—hence, an essentially new story. It marked the beginnings of modern transport, by land and air; the great development of the electrical and eventually of the electronics industries; and, finally, the revolution that is stemming from control of the atom, of a magnitude not yet fully realized.

For this and similar reasons it was wise to end this huge project at the beginning of our own era. We must simply be grateful to the editors and the many writers for having so expertly and attractively produced these five outstanding volumes. Finally, a special vote of thanks must be extended to the Imperial Chemical Industries Limited, without whose thoughtful foresight and support this *History* would never have been written.

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East and West in India's Development. Wilfred Malenbaum. National Planning Association Washington, D.C., 1959. xi + 67 pp. $1.75.

The National Planning Association's project on the economics of coexistence was initiated in 1956 to investigate Soviet trade-and-aid programs in uncommitted countries of Asia and the Middle East and to evaluate, if possible, both the impact of this Soviet "competitive coexistence" drive and the capability of the Communist bloc for further expansion. As a preliminary to the preparation of a more general analysis, the project commissioned a series of studies of countries and areas, of which this one on India is the third to be published.

The crux of the Indian problem, according to Malenbaum, is whether or not the country can modernize itself by democratic means—in contrast to the totalitarian techniques used by the Soviet Union and China—and thus not only preserve its own form of government but also set an example for the rest of the underdeveloped world. To date, Malenbaum points out, the results have been mixed: progress during the first Five Year Plan was encouraging, but the country's financial resources were insufficient for the ambitious targets of the Second Plan period that began in 1956. Despite stepped-up aid from the West, the plan targets had to be cut back to a hard core. Now the question is whether this limited success, achieved with great effort, is the best that can be hoped for, and whether it is perhaps a Pyrrhic victory.

The Soviet Union, appearing on the scene in India at a critical moment with its own "trade-and-aid" program, emphasized what underdeveloped countries consider to be the hard prerequisite for rapid industrialization: steel, and other heavy industry, in addition to exploration of resources. This accent on hard prerequisites, according to Malenbaum, may warp the pattern of future development in India in two ways: by lessening the emphasis on greater productivity and by letting the effects of the exchange shortage fall most heavily on private industry, which has surprised the planners by a rigorous initiative that exceeded expectations and targets.

What is at stake, then, is not simply a minimal success but also the long-run prospects for economic development in India. The Soviet bloc might well benefit either from total failure in India or from resort to more authoritarian methods during a successful drive toward modernization. The West, on the other hand, can benefit only if a balance is maintained between economic achievement and democratic method.

Under these circumstances, Malenbaum feels, it would be highly desirable for the West to initiate, with India, a coordinated effort for ensuring a broad balance of development, both within the Second Five Year Plan and within the design for the forthcoming Third Five Year Plan. This is an argument which, in its own interests, the West cannot afford to ignore.

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Excursion Flora of the British Isles. A. R. Clapham, T. G. Tutin, E. F. Warburg. Cambridge University Press, New York, 1959. xxxiii + 579 pp. $4.50.

This is a condensation of the same authors' *Flora of the British Isles,* and, like that excellent volume, this one has already found an enthusiastic audience.

Artificial keys to family groups, keys to genera and to species, short descriptions of "all species that are generally common in lowland districts of the British Isles," a glossary, and an index make up the contents. Omitted from the *Flora* are text figures and descriptions of the less common species, as well as data of interest principally to the professional botanist.

The typography is exceptionally clear, and because of the light-weight paper used, this is a very small volume which will easily fit into the field packs and knapsacks of the astonishingly large number of amateur field botanists who study Britain's flora.

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Smithsonian Institution

Program for College Preparatory Mathematics. Report of the Commission on Mathematics. College Entrance Examination Board, Princeton, N.J., 1959. 63 pp. + *Appendices* (bound separately). 231 pp. $1 each.

The Commission on Mathematics was established by the College Entrance Examination Board in 1955 because "it felt that curricular reform in secondary school mathematics was long overdue, and that the Board, as an agency representing both colleges and secondary schools, could and should use its influence to improve the current situation." Accordingly, the commission, composed of representatives from universities and secondary schools and with the financial support of the Carnegie Corporation, presents, after intensive scrutiny and study, a proposed new program for secondary-school mathematics.

The commission does not claim that this is *the* new program, nor does it believe that a sudden change is either practicable or desirable. But this report does indicate the lines along which the commission feels progress should be made.

After describing the urgent need for curricular revision and stating the premises, the commission's report outlines the prerequisite mathematics assumed, gives proposed sequences for grades 9 through 12, and discusses the vital role of teacher education and the articulation of school and college mathematics.

In brief, they summarize their proposed program as follows: "1. Strong preparation, *both* in concepts *and* in skills, for college mathematics at the level of calculus and analytic geometry 2. Understanding of the nature and role of deductive reasoning—in algebra as well as in geometry 3. Appreciation of

mathematical structure ('patterns')—for example, properties of natural, rational, real, and complex numbers 4. Judicious use of unifying ideas—sets, variables, functions, and relations 5. Treatment of inequalities along with equations 6. Incorporation with plane geometry of some coordinate geometry, and essentials of solid geometry, and space perception 7. Introduction in grade 11 of fundamental trigonometry—centered on coordinates, vectors, and complex numbers 8. Emphasis in grade 12 on elementary functions (polynomial, exponential, circular) 9. Recommendation of additional alternative units for grade 12: *either* introductory probability with statistical applications, *or* an introduction to modern algebra."

Acompanying this report is a publication called *Appendices,* in which some of the topics listed in the report are described in more detail.

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A New Method in the Theory of Superconductivity. N. N. Bogoliubov, V. V. Tolmachev, D. V. Shirkov. Translated from the Russian. Consultants Bureau, New York; Chapman and Hall, London, 1959. 121 pp. $5.75.

In the spring of 1957 came a major break-through in the microscopic theory of superconductivity with the announcement of the theory of Bardeen, Cooper, and Schrieffer. Almost immediately a stream of papers by N. N. Bogoliubov and his coworkers appeared, reflecting the fact that the preliminary announcements of the "BCS" theory indicated certain formal similarities between it and Bogoliubov's 1947 theory of superfluidity in liquid helium.

This book, completed in Russian in January 1958, is the culmination of this work—a synthesis of the previously published results and of several new contributions. As such, it is more a report of active research than a review of a body of theory that has withstood the test of time. Being highly technical and debatable, it is intended only for the specialist. A slightly condensed English version of the original Russian book, prepared in the Soviet Union, appeared in the *Fortschritte der Physik* and may be obtained from the authors, in preprint form, for the asking.

After a lucid introduction devoted mainly to a résumé of the 1947 superfluidity theory, three chapters are given to the Fröhlich model, in which the electron-phonon interaction is retained but the Coulomb repulsion between electrons is omitted. The treatment of the electrons is equivalent to that of Bardeen, Cooper, and Schrieffer, although mathematically it is much simpler, more elegant and convenient. The lattice is given a parallel treatment which is somewhat more satisfying and systematic, although the advantage of better convergence claimed for this procedure is not explicitly shown. The collective motions are also investigated by a method incorporating the features of recent work by Gell-Mann, Brueckner, Sawada *et al.* Unfortunately the omission of Coulomb repulsions is crucial here, and so the results are somewhat misleading.

In two later chapters appears for the first time Shirkov's formidable attempt to include the Coulomb repulsions. The development is formal and includes a number of approximations, some of which are neither discussed nor made explicit. Here the complete text is very helpful, since some material essential for understanding the mathematical details is omitted in the *Fortschritte* version. Among the results of this investigation are a less restrictive criterion for superconductivity than that of Bardeen, Cooper, and Schrieffer and the loss of the isotope effect—results which seem at best questionable.

In the seventh chapter Tolmachev studies the conditions for superconductivity in a many-electron system with general weak interactions, but without phonons. He shows that the "reduced Hamiltonian" is sufficient for study of the ground state and he also includes a treatment of collective motions in the presence of Coulomb interactions. Some of this analysis of collective motions has since been shown to be wrong. The concluding chapter shows that the partition sum calculated by Bardeen, Cooper, and Schrieffer is exact for the reduced Hamiltonian problem. This chapter also contains some comments on the electrodynamics but no treatment of the Meissner effect and the problem of gauge invariance, and no applications of the theory to specific problems.

Thus, this book contains several lasting contributions to the fundamental theory, together with some doubtful ones, but not the "unprecedented *complete* solution" described on the jacket.

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Progress in the Chemistry of Organic Natural Products. vol. 15. L. Zechmeister. Springer, Vienna, Austria, 1958. 244 pp. $9.75.

The 15th volume of this well-known review series covers four subjects for which review articles of this type are very timely.

The first review, written in German by Von H. H. Schluback, covers carbohydrate metabolism in the grasses. Although it is not a long review (30 pages), methods of isolation, analysis, molecular weight determination, and structural study are treated. Since the review is written primarily from the standpoint of those interested in agriculture and the production of food, data are given to show the change in carbohydrate and protein content during the growing period.

The second review, written in English by L. Zechmeister, is concerned with in vitro conversions of naturally occurring carotenoids. Although it was written to cover only a small segment of the chemistry of the carotenoids, it gives much information (in 52 pages) about the field in general, the nature of these substances, and the tools available for their study. N-Bromosuccinimide, a reagent investigated in recent years for many transformations in other fields, is shown to be a very useful reagent for the carotenoids. The same can be said of boron trifluoride, which forms a complex that yields useful and specific transformation products when treated in the proper way. Chromatography and spectroscopic examination of the fractionated products make it possible to interpret the transformations in a way very satisfying to the experimentalist.

The third review, on the chemistry of *Podophyllum,* written in English by J. L. Hartwell and A. W. Schrecker, gives (in 83 pages) an excellent coverage of the subject from the standpoint of organic chemistry. The interest in *Podophyllum,* which arose before chemistry was a science, has continued to the present—an interest aroused by a number of its demonstrated or alleged physiological properties. No therapeutic effect has been unequivocally demonstrated except in the case of condyloma acuminatum. However, certain of the drug's cytological effects have been more interesting for recent investigators. The type of structures found for the active principles is not unique as far as natural products are concerned, nor is any unusual experimental approach required for their study.

The fourth review, written in English by Dorothy Crowfoot Hodgkin, is a truly significant treatise, from a number of standpoints. It gives a concise account of the experimental data and the reasoning by which the investigators arrived at the complicated structure of vitamin B_{12}, and it gives, in addition, a good appraisal of the present state of development of the tools available—x-ray analysis in particular—for solving such a formidable problem. Modern isolation techniques are reasonably adequate for study of a fairly complex structure, whether or not the substance of interest will crystallize. However, the organic chemist has always preferred crystalline preparations, and this account shows how important it can be to obtain crystals of a derivative suitable for x-ray study. In the case of vitamin B_{12} it was possible to deduce the major features of the whole structure by x-ray studies, after approximately half the structure had been revealed by the methods of organic chemistry.

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Economics of American Forestry. Albert C. Worrell. Wiley, New York; Chapman and Hall, London, 1959. x + 441 pp. Illus. $9.75.

Integrating subject matter from the social sciences with that from the natural sciences is one of the most challenging and vital assignments in university teaching. Worrell's new book will be a valuable aid to those who undertake to combine the principles of economics and the practice of forestry.

Written for undergraduate forestry students, specifically for students who know something about forestry but very little about economics, the book analyzes the operations of people who are engaged in producing or enjoying the products of forests. To do this, the author uses a multitude of practical examples from almost every phase of forestry activity.

Within the framework and terminology of economic theory, the book discusses such topics as why people own forest land, why they manage it as they do, why they operate their sawmills and wood-procurement facilities as they do, why others buy forest products, and how product prices are determined and what makes prices change. It also suggests ways in which economic analysis can be used to determine more effective methods of using land, manpower, and capital in forestry activities.

The book effectively introduces the reader to the economics of forest production, marketing, valuation, and consumption. It also deals briefly with price theory and land economics. But with such a broad scope, the depth of treatment is necessarily shallow, and this quite clearly separates prospective readers into two groups. The book will be enthusiastically received by the student or practicing forester who is interested in an introduction to the workings of the forest economy. But the student, practicing forester, or researcher who wants a working knowledge of the tools of economic analysis must look beyond this text; this group will have to go directly to economics texts and journal articles.

Perhaps the most important single contribution of this book is the effective way in which it presents forestry in a broad frame of reference—giving forestry students a view of the people they are producing for, as well as information about the trees they are working with. It should also enable students to see more clearly how silviculture, protection, management, utilization and other forestry specialties fit together—how knowledge from each is needed if forest resources are to provide the products that people want most from them.

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Miscellaneous Publications

(Inquiries concerning these publications should be addressed, not to Science, *but to the publisher or agency sponsoring the publication.)*

Activities of the National Institutes of Health in the Field of Gerontology, January 1959. Center for Aging Research, National Institutes of Health, Bethesda 14, Md. 121 pp.

Adsorption, Dialysis, and Ion Exchange. Chem. Eng. Progr. Symp. Ser., No. 24, vol. 55, 1959. American Inst. of Physics, New York, 1959. 219 pp.

American Academy of Arts and Sciences, Records, 1958–1959. The Academy, Boston, Mass., 1959. 122 pp.

The Biographical Approach to John Dalton. Frank Greenaway. Portico Library, Manchester, England, 1959. 98 pp. £1 1s.

Chemistry and Biology of the Starch Granule. vol. 2 of *Protoplasmatologia, Handbuch der Protoplasmaforschung.* N. P. Badenhuizen. Springer, Berlin, 1959. 74 pp. $5.95.

Differenciation des cellules sexuelles et fecondation chez les Phanerogames. vol. 7 of *Protoplasmatologia, Handbuch der Protoplasmaforschung.* Bernard Varzart. Springer, Berlin, 1958. 158 pp. $10.50.

Educators Guide to Free Films. Compiled and edited by Mary Foley Horkheimer and John W. Diffor. Educators Progress Service, Randolph, Wis., ed. 19, 1959. 639 pp. $6.

Frost, Drought, and Heat Resistance. vol. 8 of *Protoplasmatologia, Handbuch der Protoplasmaforschung.* J. Levitt. Springer, Berlin, 1958. 87 pp. $5.95.

Die Gefässarchitektur der Niere. A. Von Kügelgen, B. Kuhlo, W. Kuhlo, Kl.-J. Otto. Thieme, Stuttgart, Germany, 1959. 111 pp. $8.95.

Laboratory Manual for General Bacteriology. Compiled by George L. Peltier, Carl E. Georgi, Lawrence F. Lindgren. Wiley, New York; Chapman & Hall, London, ed. 5, 1959. 295 pp. $4.50.

Nuclear Engineering pt. 5. Chem. Eng. Progr. Symp. Ser. No. 22, vol. 55. American Inst. of Physics, New York, 1959. 199 pp.

Ontogeny of the Inflorescence and the Flower in Drimys Winteri Var Chilensis. Publ. in Botany, vol. 30, No. 4, Shirley Cotter Tucker. Univ. of California Press, Berkeley, 1959. 80 pp. $1.50.

Publications of Field Museum of Natural History. Finding index. Zoological Ser., vol. 25, pt. 4. Reuben Myron Strong. Field Museum of Natural History, Chicago, 1959. 185 pp. $2.75.

Radioisotopes in the Service of Man. Fernand Lot. United Nations Educational, Scientific and Cultural Organization, Paris, 1959 (order from Columbia Univ. Press, New York). 84 pp. $1.

Les Regulations Physiologiques. Essai de revision biométrique du problème de l'homéostasie. Eugene Schreider. Office International de Documentation et Librairie, Paris, 1958. 89 pp.

A Review of the Genus Hoplomys *(Thick-Spined Rats), with Description of a New Form from Isla Escudo de Veragus, Panama.* Misc. Collections, vol. 139, No. 4. Charles O. Handley, Jr. Smithsonian Institution, Washington 25, 1959. 10 pp.

The Schoolhouse Disasters. Family and community determinants of the child's response to disaster. NAS-NRC Publ. No. 554. Helen Swick Perry and Stewart E. Perry. National Acad. of Sciences–National Research Council, Washington 25, 1959. 66 pp. $1.50.

Science for the Academically Talented Student in Secondary School. Robert R. Donaldson, chairman, National Education Assoc., Washington, D.C. 63 pp. $0.60.

Series of Syllabi in Social Gerontology. No. 1, "The economics of an aging population," Walter H. Franke and Richard C. Wilcock, 57 pp.; No. 2, "The psychological aspects of aging," Raymond G. Kuhlen and Woodrow W. Morris, 30 pp.; No. 3, "The sociology of aging and the aged," Irving L. Webber and Gordon F. Streib, 35 pp.; No. 4, "Social welfare and the aged," Gordon J. Aldridge and Fedele F. Fauri, 51 pp.; No. 5, "An interdisciplinary course in social gerontology," Bernice L. Neugarten, Robert J. Havinghurst, Claire F. Ryder, 40 pp. Inst. for Social Gerontology, Univ. of Michigan, Ann Arbor, 1959.

Reports

Monoamine Oxidase, Psychoenergizers, and Tranquilizers

Abstract. 1-Benzyl-2-methyl-5-methoxytryptamine (BAS) inhibits monoamine oxidase in man. This finding confirms the findings of Woolley in mice. Since BAS, a tranquilizing agent, and Marsilid, a psychoenergizer, are both monoamine oxidase inhibitors, doubt is cast upon the hypothesis that the stimulatory effect of Marsilid is due to its ability to inhibit monoamine oxidase.

Marsilid has been shown to be an inhibitor of monoamine oxidase (*1*). The attractive hypothesis has been put forth that the antidepressant action of Marsilid is due to its ability to inhibit the enzyme (*2*, *3*) and, more specifically, that the increased level of brain serotonin or norepinephrine is responsible for the central stimulatory effects of Marsilid (*4*). A parallelism between the enzymatic inhibition and clinical effectiveness of Marsilid and its analogs has been reported (*3*); Resnick (*5*), however, reported a lack of correlation between the degree of enzymatic inhibition and the clinical effectiveness of Marsilid in several patients.

Woolley *et al.* (*6*) reported that 1-benzyl-2-methyl-5-methoxytryptamine (BAS) is a monoamine oxidase inhibitor in mice. We have confirmed this finding in man. In our first study, BAS (100 mg/day) was administered orally to four schizophrenic patients for the first 2 weeks. During the third and fourth weeks, BAS and D,L-5-hydroxytryptophan (5-HTP) (30 mg/day) was administered intramuscularly. BAS alone did not affect the excretion of endogenously formed 5-hydroxyindoleacetic acid (5-HIAA), but it did prevent the expected increase of 5-HIAA due to the metabolism of 5-HTP in three of the four patients; only 40 percent of the L-5-HTP was recovered in the fourth patient. In our second study, two patients received BAS for 1 week and BAS and 5-HTP (100 mg/day), intramuscularly, for the next 3 weeks. Again the BAS prevented the expected increase in urinary 5-HIAA. This was confirmed by a two-dimensional paper chromatogram which showed the usual amount of 5-HIAA and a large increase in serotonin and 5-HTP, presumably the D-form.

Although BAS and Marsilid are both monoamine oxidase inhibitors, BAS, unlike Marsilid, has tranquilization properties. Woolley and his co-workers (*7*) first observed the tranquilizing action of BAS in mice. Wilkins *et al.* (*8*), in a study of BAS on patients with hypertension, noted a state of sedation and tranquilization. Rudy and his co-workers (*9*) administered BAS to 24 moderately disturbed, chronic psychotic female patients and reported "a strong tranquilization action not unlike that of reserpine."

Thus we have a new and very interesting situation of two monoamine oxidase inhibitors, one a psychoenergizer (Marsilid), the other a tranquilizer (BAS). This finding casts some doubt on the hypothesis that Marsilid exerts its central stimulatory action by virtue of its ability to inhibit monoamine oxidase. It should be noted, however, that although BAS does possess central activity, it does not readily pass into the brain (*7*) and, further, that we have measured the inhibition of monoamine oxidase in the whole organism and not in the brain specifically.

It is relevant that isoniazid (Rimifon) is not a monoamine oxidase inhibitor (*1*) but that it nevertheless possesses central stimulatory properties in man (*10*) and has been used successfully in the treatment of depression (*11*). Since Rimifon is closely related structurally to Marsilid, it seems likely that a common mechanism underlies the psychoenergizing properties of Marsilid, Rimifon, Marplan, and other analogs. The possibility that the central stimulatory action of these compounds is due to their ability to produce a pyridoxine deficiency (*12*), to inhibit decarboxylase activity (*13*), and to inhibit transaminase activity (*14*) should be considered (*15*).

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References and Notes

1. E. A. Zeller, J. Barsky, E. R. Berman, J. R. Fouts, *J. Lab. Clin. Med.* **40**, 965 (1952).
2. H. P. Loomer, J. C. Saunders, N. S. Kline, paper presented at regional research conference, Am. Psychiatric Assoc., 6 Apr. 1957, Syracuse, N.Y.; N. S. Kline, *J. Clin. Exptl. Psychopathol. & Quart. Rev. Psychiat. Neurol.* **19**, 72 (1958).
3. ———, S. C. Saunders, S. A. Bailey, E. Gosline, M. Vaisberg, paper presented at meeting of N.Y. Acad. Sci., 20–22 Nov. 1958, New York, N.Y.; A. Voelkel, *ibid.*
4. P. A. Shore, *J. Clin. Exptl. Psychopathol. & Quart. Rev. Psychiat. Neurol.* **19**, 56 (1958).
5. O. Resnick, J. M. Wolfe, H. Freeman, F. Elmadjian, *Science* **127**, 1116 (1958).
6. D. W. Woolley and P. M. Edelman, *ibid.* **127**, 281 (1958).
7. D. W. Woolley, E. Van Winkle, E. Shaw, *Proc. Natl. Acad. Sci. U.S.* **43**, 128 (1957).
8. R. W. Wilkins and W. Hollander, *Circulation* **16**, 256 (1957).
9. L. H. Rudy, E. Costa, F. Rinaldi, H. E. Himwich, *J. Nervous Mental Disease* **126**, 284 (1958).
10. R. H. Reilly, K. F. Killam, E. H. Jenney, W. H. Marshall, T. Tansig, N. S. Apter, C. C. Pfeiffer, *J. Am. Med. Assoc.* **152**, 1317 (1953).
11. J. A. Flaherty, *Delaware State Med. J.* **24**, 198 (1952); J. Delay and J. F. Buisson, *J. Clin. Exptl. Psychopathol. & Quart Rev. Psychiat. Neurol.* **19**, 51 (1958); H. M. Salzer and M. L. Lurie, *A.M.A. Arch. Neurol. Psychiat.* **70**, 317 (1953); ———, *Ohio State Med. J.* **51**, 437 (1955).
12. J. P. Biehl and R. W. Vilter, *J. Am. Med. Assoc.* **156**, 1549 (1954); *Proc. Soc. Exptl. Biol. Med.* **85**, 389 (1954).
13. F. Rosen, paper presented at meeting of N.Y. Acad. Sci., 20–22 Nov. 1958; A. N. Davidson, *Biochim. et Biophys. Acta* **19**, 131 (1956).
14. H. Weissbach, W. King, A. Sjoerdsma, S. Udenfriend, *J. Biol. Chem.* **234**, 81 (1959).
15. We thank D. W. Woolley for suggesting the study and for supplies of BAS and 5-hydroxytryptophan. We acknowledge completion of the work under a grant from the Ford Foundation and the Scottish Rite Committee.
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24 June 1959

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

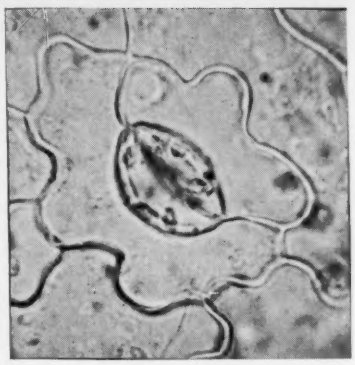
Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

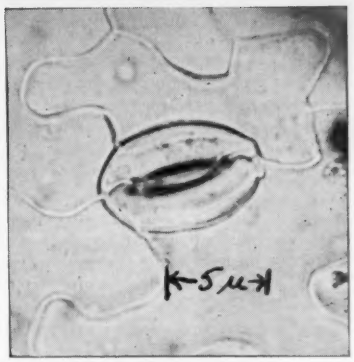
For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

Disappearance of Guard Cell Chloroplasts in Ultraviolet-Irradiated Leaves

Abstract. Ultraviolet irradiation of kidney-bean leaves results in the disappearance of chloroplasts from guard cells. The evidence indicates that ultraviolet irradiation causes plastid breakdown indirectly through an effect on guard-cell metabolism.

In some earlier experiments (*1*) the disappearance of guard-cell chloroplasts in ultraviolet-irradiated bean leaves was observed, but not reported. Cells of irra-






Stomata of red kidney bean leaf epidermis after incubation in the dark: Fig. 1 (left), 3 hours after ultraviolet irradiation; Fig. 2 (middle), 24 hours after ultraviolet irradiation; Fig. 3 (right), unirradiated control.

diated epidermis left in the dark die (*2*), and in our experiments, after 4 days, when the epidermal strips were made, the epidermis had undergone considerable decomposition. The disappearance of chloroplasts could therefore have been related to this decomposition rather than to more immediate effects of ultraviolet treatment. Experiments were therefore designed to obtain more information on the mechanism of ultraviolet-induced disappearance of guard-cell chloroplasts (*3*).

Primary leaves were detached from greenhouse-grown Red Kidney bean seedlings just before expansion of the first trifoliate leaf. Half-leaves were cut free of the midrib and placed, lower surface up, on moist filter paper in petri dishes. A glass slide was placed across the center of each half-leaf, and the leaves were irradiated for 2 minutes at 36 cm with a General Electric (G8T5) germicidal lamp, giving 130 μw/cm² (2537 A) at the leaf surface. The glass slides were then removed, and the leaves were either placed immediately in the dark at 22°C for varying periods or illuminated with 680 ft-ca of light from "daylight" fluorescent tubes for 4 hours before incubation in the dark.

In the unprotected portions of leaves, the glazing which is a characteristic external symptom of injury (*1*) never was evident until 24 hours after incubation in the dark. However, epidermal strips taken from irradiated surfaces as early as 3 hours after treatment already showed indications of chloroplast damage in guard cells when examined microscopically. As shown in Fig. 1, the outlines of such plastids are extremely irregular or angular in appearance. At the end of 24 hours most guard cells have no chloroplasts, although an occasional guard cell shows faintly visible structures with the general outlines of plastids. Both types are represented around the stoma in Fig. 2. In Fig. 3, guard cells with normal chloroplasts are pictured. These were characteristic of epidermal strips taken from leaves prior to irradiation, of portions of irradiated leaves protected by the glass slide, or of unprotected portions of such leaves which were illuminated with visible light for 4 hours after irradiation. All the photographs were made in ordinary light, but examination of chloroplast-free guard cells with phase optics did not reveal structures resembling chloroplasts; this indicates that ultraviolet irradiation leads to an actual disintegration of chloroplasts, rather than merely to changes in light absorption or index of refraction.

Along with lysis, chloroplasts lost starch, as indicated by iodine staining before and after the structural changes occurred. In one experiment, epidermal strips rather than leaves were irradiated. This also resulted in the disappearance of chloroplasts, but not in strips floated in 0.1-percent $HgCl_2$ after exposure to ultraviolet irradiation.

Guard-cell chloroplasts in bean, as in other species studied (*4*), are lighter in color than chloroplasts in the mesophyll; they are also less stable, since aside from the effect of ultraviolet irradiation, when leaves are boiled or strips are steamed for 1 minute, or even if leaves are kept at 33°C for 24 hours, guard-cell chloroplasts break down, while mesophyll chloroplasts show no visible damage immediately after such treatment.

Some preliminary experiments were also done with geranium, with results similar to those in bean, and with calla lily, which showed no visible effects after ultraviolet irradiation equivalent to that used in studying guard-cell chloroplasts.

The disappearance of guard-cell chloroplasts so soon after ultraviolet treatment obviously cannot be attributed to secondary effect of tissue decomposition. Whether ultraviolet irradiation acts directly to disrupt chloroplasts or indirectly by influencing cell metabolism cannot be finally decided on the basis of our work. Ultraviolet irradiation produces a depolymerization of nucleic acid (*5*) and proteins (*6*) and, were it acting similarly on plastids, the diffusion of fragments would be expected to be relatively independent of subsequent treatment. Use of $HgCl_2$ prevented chloroplast breakdown in our experiments; this might have been due to its action as a fixative. However, the disappearance of starch along with chloroplasts suggests that ultraviolet irradiation acts indirectly by at least temporarily favoring degradative over synthetic metabolism (*6*), and that the Hg ion inhibits the metabolism associated with injury from irradiation.

Pertinent to our findings are some cytological effects of ultraviolet irradiation reported by others. These effects include the disruption and subsequent coalescence of the large mitochondria of *Saccharomyces* (*8*), the protoplasmic fragmentation of the ciliated protozoan *Spirostomum ambiguum* (*9*), and the contraction and breaking up of the ribbon-like chloroplast of *Spirogyra* (*10*). In none of these cases, however, was total disappearance of the damaged structure reported.

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References and Notes

1. M. Chessin, *Ann. Appl. Biol.* **46**, 88 (1958).
2. F. C. Bawden and A. Kleczkowski, *Nature* **169**, 90 (1952).
3. This study was supported by National Institutes of Health grant E-596(C4).
4. F. E. Lloyd, *Carnegie Inst. Wash. Publ. No. 82* (1908), p. 139.
5. A. Hollaender, J. P. Greenstein, W. V. Jenrette, *J. Natl. Cancer Inst.* **2**, 23 (1941).
6. T. Svedberg and S. Brohult, *Nature* **143**, 938 (1939).
7. P. C. Owen, *ibid.* **180**, 610 (1957).
8. A. Sarachek and G. F. Townsend, *Science* **117**, 31 (1953).
9. E. S. Shirley, Jr., and H. E. Finley, *Trans. Am. Microscop. Soc.* **68**, 136 (1949).
10. E. N. Harvey, *J. Gen. Physiol.* **25**, 431 (1942).

23 March 1959

Metabolism of the Gerbil, Meriones unguiculatus

Abstract. The critical temperature for the Mongolian gerbil, *Meriones unguiculatus*, is determined to be 30°C. The zone of thermal neutrality extends to 40°C. This species possesses a high degree of tolerance for heat and has a greater capacity for temperature regulation than is reported for many of the desert rodents.

The small desert rodents of the genus *Meriones* are widely distributed throughout North Africa, southeast Russia, Asia Minor and southwest Asia. The species used in this study was *Meriones unguiculatus*, first described by Milne-Edwards in 1867 (*1*). Milne-Edwards' nomenclature was verified by Chatworth-Musters and Ellerman in 1947 in their revision of the genus (*2*). The locality where Milne-Edwards collected his type specimens of this species is given as an area north of Peking and west of a line drawn between Mukden and Harbin, corresponding on a modern map to northwest Manchuria near the Mongolian plateau. Members of this genus dig elaborate underground retreats, often at two levels, including numerous chambers for both storage and nesting. The chambers are interconnected, with a labyrinth of runways (*3*). Very little is known of the physiology of this genus. A small number of gerbils were available, and it was thought worth while to ascertain the oxygen consumption of these under controlled conditions.

The oxygen consumption was measured by the closed-circuit method. Nine animals were used, three males and six females, ranging in weight from 61 to 80 g. They were housed individually during the course of the experiments in glass cylinders large enough to allow them to move back and forth and to turn around. A wire screen separated the animals from the bottom of the tube, which was covered with soda-lime for CO_2 absorption. The cylinder was immersed in a thermostatically controlled water bath and connected to a manometer and to an oxygen supply. The animals, after being introduced into the chamber, were allowed about 1 hour in which to become quiet. During this time a flow of air was maintained through the cylinder. When the animal appeared to be in a resting state, the air flow was interrupted and the experimental period was begun. The oxygen used was fed from an oiled graduated syringe. The pressure within the test chamber was maintained at the pressure of the external atmosphere.

The experimental periods were usually limited to a length of time necessary for the animal to consume 50 ml of oxygen. Before a given measurement was accepted as valid, it was required that at least two consecutive runs agree closely. Metabolic measurements were conducted at ambient temperatures ranging from 15° to 40°C, as measured in the water bath. All gas volumes were corrected to standard conditions of temperature and pressure. The metabolic rate was expressed as oxygen consumption in milliliters per gram of body weight per hour. Since the animals were awake and quiet, but not fasting, this quantity should be regarded as the resting, not the basal, metabolism.

From the results presented in Fig. 1, it is estimated that the critical temperature for *Meriones unguiculatus* is approximately 30°C and that the regression equation for the points below 30°C is

$$\text{ml of } O_2/\text{g hr} = 5.660 - 0.141\,^\circ\text{C}$$

It must be concluded that the zone of thermal neutrality extends to 39°C, since no significant regression could be demonstrated between 30° and 40°C. The term *critical temperature* is used here in its original sense: the lowest ambient temperature at which the animal remains in a basal or resting metabolic condition. At temperatures above 35°C these animals appeared to be severely affected, showing increased respiratory rate, panting, matted fur, and general demeanor indicative of impending collapse. One animal died after about 1 hour's exposure to a temperature of 40°C. It was for this reason that few measurements were made at temperatures above 35°C.

The comparatively wide zone of thermal neutrality observed in this series of experiments is in contrast to the narrow zones described by Herrington (*4*) for the laboratory rat, mouse, and guinea pig. Herrington found the neutral zone for the rat to be 28.0° to 28.9°; for the guinea pig, 30.0° to 30.9°; and for the mouse, 31.0° to 31.9°C. Similarly, Dawson (*5*) found a fairly restricted range of neutrality in the two species of kangaroo rat and in the antelope ground squirrel he examined. The latter animal exhibited no sign of distress when its body temperature was elevated to 42.4°C. If, as Scholander (*6*) points out, a distinguishing physiological characteristic of most tropical animals is a limited and transitory zone of thermal neutrality, one is justified in doubting that *Meriones unguiculatus* is a true desert rodent. The individuals used in our study came from an area which, as far as can be ascertained, is temperate, with well-defined seasons, a moderate cover of vegetation, and cold winters. Other members of the same species from a hotter, dryer area such as North Africa may exhibit different physiological responses to the same thermal load.

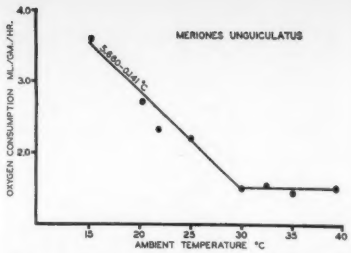

Fig. 1. Effect of ambient temperature on oxygen consumption.

Since these animals would not accept a rectal thermocouple for any length of time, it was not feasible to obtain their body temperatures and make metabolic measurements simultaneously. Therefore, after the metabolic measurements had been completed, some of the animals were reexposed to these ambient tempertures in a hot room, and their body temperatures were recorded over 5-hour periods.

In the hot room, the gerbils were individually housed in large plastic cages and supplied with water; however, they were not seen drinking during any of the exposures, and it is believed that they did not drink. Rectal temperatures were taken hourly with a thermocouple probe, care being taken that the tip was inserted deeply enough to obtain a true reading of the core temperature. Exposures at each of the three ambient temperatures chosen, 30°, 35°, and 40°C, were carried out, on different days. Table 1 summarizes the results of these exposures. The 2-hour value is included to show the thermal state of the animal at the end of a metabolic experiment which usually took 2 hours.

It is apparent that an ambient temperature of 40°C for a period of 5 hours is well tolerated by the gerbil. Although the animals were observed to lie quietly in an extended position for the duration of their exposure to this temperature,

Table 1. Summary of the results of exposure of gerbils to different degrees of heat.

Ambient temperature (°C)	Relative humidity	Body temperature (°C)				
		Mean control at 23°C ambient	After exposure to heat		Change	
			2 hr	5 hr	2 hr	5 hr
30	50	38.2	38.1	38.4	−0.1	0.2
35	40	38.6	38.7	39.3	0.1	0.7
40	30	38.4	39.3	39.9	0.9	1.5

they exhibited no other overt signs of discomfort.

The salivation and fur-licking behavior described by Herrington (*4*) for the mouse, rat, and guinea pig, and by Schmidt-Nielsen (*7*) for the cat and rabbit, was at no time apparent in the gerbil upon exposure to heat. The distress and near-prostration that was seen when the gerbils were in the metabolic chamber at 40°C was doubtless due to the saturated atmosphere within the tube, as evinced by the film of moisture that appeared on the sides. Adolph (*8*) pointed out some time ago that evaporative cooling is one of the principal factors in determining an animal's tolerance of heat.

It is of interest that the gerbil, *Meriones unguiculatus,* has a greater capacity for temperature regulation under heat conditions than either of the two species of kangaroo rats or the antelope ground squirrel observed by Dawson (*5*). Unfortunately, there are published in the available literature no observations on the habits of *Meriones unguiculatus* in a natural state; with the degree of heat tolerance this species apparently possesses, it is likely that these animals would be able to spend a good part of their time in activity outside of the burrow during daylight hours.

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References

1. A. Milne-Edwards, *Ann. sci. nat.* (*Zool.*) 7, 375 (1867).
2. J. L. Chatworth-Musters and J. R. Ellerman, *Proc. Zool. Soc.* (*London*) 117, 478 (1947).
3. *Traité de Zoologie* (Masson, Paris, 1955), vol. 17, fasc. 2.
4. L. P. Herrington, *Am. J. Physiol.* 129, 123 (1940).
5. W. R. Dawson, *J. Mammal.* 36, 543 (1955).
6. P. F. Scholander, R. Hock, V. Walters, F. Johnson, L. Irving, *Biol. Bull.* 99, 237 (1950).
7. K. Schmidt-Nielsen and B. Schmidt-Nielsen, *Physiol. Revs.* 32, 135 (1952).
8. E. F. Adolph, *Am. J. Physiol.* 151, 564 (1947).

29 April 1959

Incorporation of Tritiated Thymidine into Meiotic Chromosomes

Abstract. In pachytene nuclei of *Melanoplus* the heterochromatin of the sex chromosome was found to synthesize DNA at a different time than the autosomal euchromatin.

Grasshoppers of the species *Melanoplus differentialis differentialis* Thomas were injected with tritiated thymidine (500 μc/ml). Each animal received 0.02 to 0.04 ml. After 2 to 7 days testes were fixed, squashed, and stained by the Feulgen method. Stripping film was applied in the usual way for autoradiographic work. During the squashing procedure, care was taken to obtain well flattened nuclei, in order to insure their intimate contact with the emulsion. The 5- and 7-day animals showed a distinct incorporation of the radioisotope into early pachytene nuclei. Thymine occurs solely in deoxyribonucleic acid (DNA), and thymidine is incorporated efficiently into DNA (*1*).

At pachytene, in the spermatocytes of *Melanoplus,* the sex chromosome forms a large block of heterochromatin which is quite distinct from the euchromatin of the autosomes. The incorporation of labeled thymidine is different for eu- and heterochromatin. In the examination of over 500 cells from five animals, four categories of nuclei were found (Fig. 1): (i) unlabeled nuclei, (ii) nuclei with only the autosomal euchromatin labeled, (iii) nuclei with grains over both eu- and heterochromatin, and (iv) nuclei with only labeling in the heterochromatic block. Due to the 1-μ resolution afforded by the tritium β-particles (*2*) and the large size of the darkly stained heterochromatic block (2.5 to 3.0 μ in diameter), the differential uptake of the isotope is clear-cut. The heterochromatin synthesizes DNA at a different time than the euchromatin, and there is an intermediate period during which the hetero- and euchromatin are synthesized either simultaneously or at close intervals.

To check accurately which of the nuclear types represents a more advanced stage, sections of the same material were made by the same procedure. The testes of *Melanoplus* animals consist of a series of follicles in which the spermatocytes are grouped in cysts. These cysts are known to be synchronized in their meiotic stages, especially during prophase. Furthermore, there is a regular anteroposterior sequence of the stages along the follicle; the younger cysts are at the

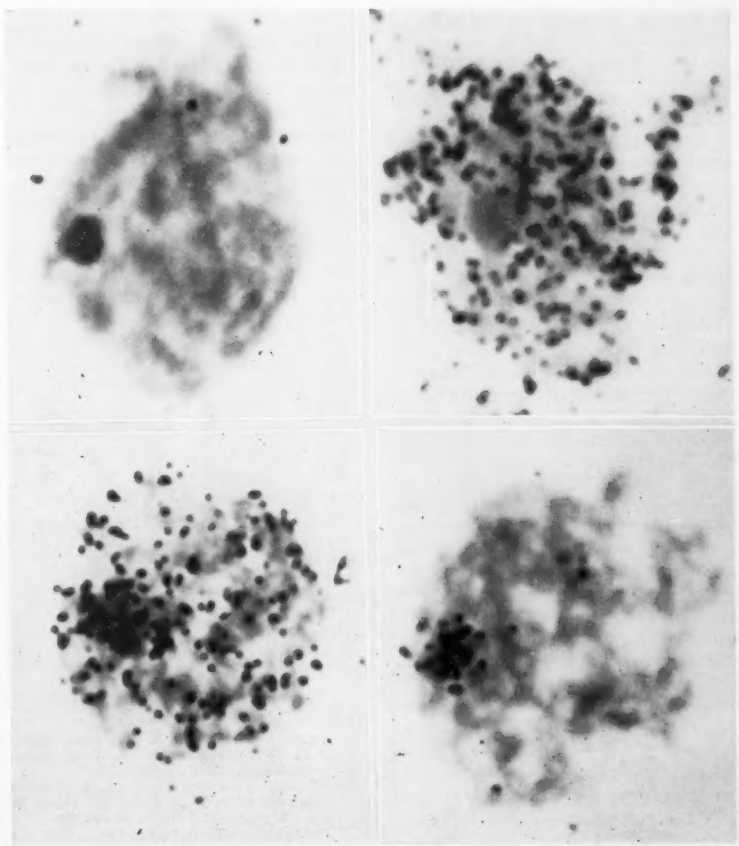

Fig. 1. Photographs of four categories of nuclei, illustrating the uptake of tritiated thymidine into early pachytene nuclei of *Melanoplus.* The sex chromosome forms a deeply stained block of heterochromatin (at the 9 o'clock position in each of the four nuclei shown). The autosomes constitute the remaining euchromatic portion of the nucleus. (Top left) Unlabeled; (top right) labeled in euchromatin only; (bottom left) labeled in both eu- and heterochromatin; (bottom right) labeling only in heterochromatin. The photograph at the top left was taken at the level of the nucleus, the others were taken at a focal level intermediate between that of the nucleus and that of grains in the emulsion (×3200).

posterior part and move successively along the follicle. The sections revealed that the nuclei with the heterochromatin labeled are the more advanced ones. Thus, the heterochromatin synthesizes DNA later than does the euchromatin (*3*).

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References and Notes

1. J. H. Taylor, P. S. Woods, W. L. Hughes, *Proc. Natl. Acad. Sci. U.S.* **43**, 122 (1957).
2. W. L. Hughes, *et al.*, *ibid.* **44**, 476 (1958).
3. The animals used in this experiment were raised from eggs obtained from Dr. T. Tahmisian of the Argonne National Laboratory, Lemont, Ill. A detailed report of these experiments is in preparation.

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25 March 1959

Ballistics of Dwarf Mistletoe Seeds

Abstract. The explosive fruit of *Arceuthobium* expels the seed for several feet, but the ballistics of seed flight has not been previously investigated. The data reported here for *A. vaginatum* f. *cryptopodum* indicate that the seeds have an initial velocity of about 1370 cm/sec and an initial acceleration of nearly 5000*g*.

The explosive fruit of the dwarf mistletoes (*Arceuthobium* spp.) is one of the most efficient mechanical seed dispersal mechanisms in any of the higher plants (*1*). As far as I know, no calculations have been made of the initial velocity or other ballistic factors of the dwarf mistletoes or any other higher plants with explosive fruits. However, Buller (*2*) studied the ballistics of the glebal masses projected by the fungus *Sphaerobolus stellatus* and found that they were thrown to a height of 14.5 ft; this indicates an initial velocity (when air resistance is disregarded) of at least 30 ft/sec (*3*).

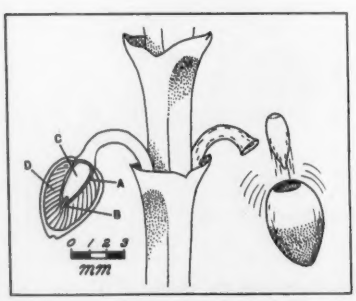

Fig. 1. Semidiagrammatic drawing of a portion of a dwarf-mistletoe shoot bearing mature fruits. Left, a longitudinal section through a fruit showing a seed (*A*), embryo (*B*), endosperm (*C*), and viscin cells (*D*). Right, a fruit immediately after the expulsion of the seed.

Each fruit of *Arceuthobium* contains a single semifusiform seed (Fig. 1). When the fruit is ripe, the pedicel is elongated and recurved so the perianth end points downward. An abscission zone develops between the tip of the pedicel and the base of the fruit. A layer of viscin cells between the seed and the exocarp of the fruit creates a considerable internal pressure, and finally the fruit is sheared from the pedicel and the exocarp contracts rapidly and hurls the seed upward (*4*). The forward end of the seed is rounded and the other end is pointed; thus, their shape approaches the ideal for the most efficient projectile.

The dwarf mistletoe used in this work (*5*) was *Arceuthobium vaginatum* f. *cryptopodum*, which is a widespread and important pathogen of ponderosa pine (*Pinus ponderosa* Laws.) in the southwestern United States. The seeds of this species average 1.1 mm in diameter and 2.9 mm in length. They are expelled for an average horizontal distance of 530 ± 30 cm, with a maximum of about 1280 cm.

The following are the experimental data obtained: Average vertical height of seeds expelled directly upward, 460 cm; terminal velocity of seeds, 750 cm/sec (*6*); average seed weight, 2.4 mg; and seed specific gravity, approximately 1.0. If it is assumed that the forces acting on the seed in flight are the force of gravity and a frictional force proportional to its velocity, then a formula may be derived relating the maximum height to which a seed goes and its initial velocity (*7*). When the data shown above are used in this formula, an average initial velocity of 1370 cm/sec or about 45 ft/sec is indicated. The kinetic energy of the seed as it leaves the fruit is thus $\frac{1}{2}\ mv^2 = 2.3 \times 10^3$ ergs.

From the initial velocity and dimensions of the seed, the time taken for the seed to leave the fruit was calculated as 4.4×10^{-4} second. The computed initial acceleration of the seed was 4.7×10^6 cm/sec^2, or nearly 5000*g*.

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References and Notes

1. H. N. Ridley, *The Dispersal of Plants throughout the World*. (Reeve, Kent, England, 1930).
2. A. H. R. Buller, *Researches on Fungi* (Longmans, London, 1933), vol. 5.
3. This calculation was based on the formula $V_0 = (2gH)^{\frac{1}{2}}$, where V_0 is the initial velocity, g is the acceleration due to gravity, and H is the height of projectile expelled directly upward.
4. L. S. Gill, *Trans. Conn. Acad. Arts Sci.* **32**, 111 (1935).
5. I wish to thank R. B. Setlow and W. R. Henson of Yale University for advising me in this work and for reviewing the manuscript, and Dr. Setlow for providing the formula given in reference (*7*).
6. The terminal velocity was determined by passing air upward through a vertical tube containing a dwarf mistletoe seed. The rate of flow necessary to suspend the seed was recorded, and the average value for 24 seeds was taken as the approximate terminal velocity.
7. $H = \frac{m^2}{c^2} g \ln \left(\frac{g + \frac{c}{m} V_0}{g} \right) - \frac{m}{c} V_0$,

when H is the maximum height of seed expelled directly upward, m is the mass of seeds, c is the ratio of frictional force (mg) to the terminal velocity (V_t), g is the acceleration due to gravity, and ln is the natural logarithm of V_0, the initial velocity.

26 March 1959

Acetylcholine Effects of γ-Carbomethoxypropyltrimethyl-Ammonium Bromide

Abstract. γ-Butyrobetaine, in comparison with its methyl ester, γ-carbomethoxypropyltrimethyl-ammonium bromide, is biologically inert. When injected into mice and insects or assayed on the frog's rectus abdominis muscle, γ-carbomethoxypropyltrimethyl-ammonium bromide has pharmacological properties resembling those of acetylcholine. Although reported to be present in rat brain during the convulsions induced by dieldrin poisoning, γ-butyrobetaine has not been found in the nervous tissue of the roach after treatment with dieldrin.

Burgen and Hobbiger (*1*) reported a similarity in the pharmacological properties of acetylcholine and the methyl ester of γ-crotonic betaine (γ-carboxyallyltrimethyl-ammonium chloride). More recently Hosein (*2*) stated that γ-butyrobetaine (GBB) was found in the brain of rats during convulsions after administration of a large dose of dieldrin. Hosein (*3*) showed that some pharmacological effects of GBB resembled those of acetylcholine. This finding is of importance, since in insects treated with chlorinated hydrocarbons, no explanation has yet been found for the manifestation of convulsions which occur in the central nervous system (*4*). Colhoun (*5, 6*) showed that after treatment of cockroaches with DDT and dieldrin a high titer of acetylcholine was found in the nerve cord at a late stage of prostration. The finding of Hosein (*3*) therefore necessitated a reevaluation of these results.

γ-Butyrobetaine was synthesized and tested for biological activity by intraperitoneal injection into mice. It was inert at the concentrations used by Hosein (*1*) and Linneweh (*7*). Further tests showed that the methyl ester of GBB, γ-carbomethoxypropyltrimethyl-ammonium bromide, had a toxicity for mice comparable to the reported toxicity of GBB injected by Hosein (*1*). Significantly, the ester was the first intermediate product in the synthesis of GBB. γ-Carbomethoxypropyltrimethyl-ammonium bromide was prepared by the reaction of anhydrous trimethylamine with methyl γ-bromobutyrate. On purification, the resulting material melted at 147° to 149°C. The actual bromide con-

Table 1. Comparison of some effects of acetylcholine and γ-carbomethoxypropyltrimethylammonium bromide.

Experiment	Acetylcholine	γ-Carbomethoxypropyltrimethyl-ammonium bromide
Intraperitoneal injection, mouse	6.7 mg/kg: slight muscular symptoms only	6.7 mg/kg: salivation, bloody tears, some convulsions, LD_{50}
Abdominal injection, roach	5 mg/g: some tremors, not toxic	5 mg/g: toxic, tremors, prostration
Electrical conduction in 6th abdominal ganglion of roach	$10^{-2}M$: no effect	$10^{-2}M$ to $10^{-3}M$: excitation, prolonged repetitive volleys
Assay of rectus abdominis muscle of frog		
Rates of activation on eserinized muscle	1 μg	30 μg
Atropine	Blocked	Blocked
Alkaline hydrolysis	No contraction	No contraction
Cholinesterase hydrolysis	No contraction	Contraction
Both substances combined	No antagonism	No antagonism
Activity on uneserinized muscle	Slight initial contraction	Contraction greater than on eserinized muscle
Cholinesterase hydrolysis (Warburg vessels)	pS optimum $10^{-2}M$ to $10^{-3}M$	No hydrolysis
Cholinesterase hydrolysis with both compounds combined	No inhibition of hydrolysis	

tent agreed with the theoretical, and the infrared absorption at 1730 and 1197 cm^{-1} is characteristic for esters of this type. The R_f was 0.20 when the compound was chromatographed on No. 4 Whatman paper with wet *n*-butanol as the developing solvent. Removal of the bromide with moist silver oxide, followed by warming and concentrating, yielded γ-butyrobetaine with an R_f of 0.03. The infrared absorption characteristics of the ester had disappeared, while a new peak appeared at 1575 cm^{-1}, corresponding to that for a carboxyl. The compounds tested for toxicity to mice were the same as those used for chromatograph, as described above.

Some of the pharmacological properties of γ-carbomethoxypropyltrimethylammonium bromide are given in Table 1. It is at once evident that, although the pharmacological effects of the compound resemble those of acetylcholine, there are distinct differences. The most striking are the effect on the uneserinized rectus abdominis muscle of the frog and the complete lack of hydrolysis of the compound by cholinesterase or in tissue breis of nerve cords of cockroaches. It is more toxic than acetylcholine when injected into cockroaches and excites electrical activity in the sixth abdominal ganglion of the ventral nerve cord of the cockroach at $10^{-2}M$ to $10^{-3}M$, whereas acetylcholine is inert at this concentration (*8*). The compound has pharmacological properties very similar to those of γ-carbomethoxyallyltrimethyl-ammonium chloride (*1*).

After treatment of cockroaches with DDT and dieldrin, a substance was found in large amounts in the nerve cord (*5*) which was correctly termed acetylcholine, for it is readily hydrolyzed by cholinesterase in vitro and in homogenates of nerve cords of roaches in which no anticholinesterase was included. However, it is thought (*5*) that the rise in acetylcholine in DDT- and dieldrin-prostrated roaches is secondary and not responsible for the convulsions that occur during the early phase of poisoning. Unless it were present in large amounts it would be difficult to detect by bioassay the occurrence of γ-carbomethoxypropyltrimethyl-ammonium bromide in the nerve cords of roaches, for this is 30 times less effective than acetylcholine. This problem is being investigated by chemical means to determine a possible primary neurological lesion in chlorinated hydrocarbon poisoning.

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References and Notes

1. H. S. V. Burgen and F. Hobbiger, *Brit. J. Pharmacol.* 4, 229 (1949).
2. E. H. Hosein, *Chem. in Can.* 10, 70 (1958).
3. ———, *Nature* 183, 328 (1959).
4. O. Giannotti, R. L. Metcalf, R. B. March, *Ann. Entomol. Soc. Am.* 49, 588 (1956).
5. E. H. Colhoun, *Can. J. Biochem. and Physiol.* 37, 259 (1959); *Chem. in Can.* 10, 66 (1958).
6. ———, unpublished data.
7. W. Linneweh, *Z. physiol. Chem. Hoppe-Seyler's* 181, 42 (1929).
8. B. M. Twarog and K. D. Roeder, *Ann. Entomol. Soc. Am.* 50, 231 (1957).

2 March 1959

Alpha-Keto Acids in Vitamin-Free Casein Hydrolyzates (Acid)

Abstract. α-Ketoglutaric and pyruvic acids were isolated as their 2-4-dinitrophenylhydrazones from five different commercial samples of vitamin-free acid hydrolyzates of casein. In addition, one sample yielded traces of α-ketobutyric acid. The hydrazones were converted by hydrogenation to glutamic acid, alanine, and α-aminobutyric acid and identified by paper chromatography.

Franck and Knoke (*1*) in 1957 reported the presence of pyruvic and α-ketobutyric acids in acid hydrolyzates of egg albumin, zein, gelatin, and casein, while Neuman and McCoy (*2*) in 1958 reported that pyruvate, oxalacetate, and α-ketoglutarate possessed growth-promoting properties with respect to isolated Walker carcinosarcoma 256 cells. These facts prompted an investigation of the α-keto acids present in commercially available casein hydrolyzates (acid) which are not uncommon components of semidefined bacteriological media.

The analysis of the α-ketoacids present involved the following procedures: (i) conversion to the 2-4-dinitrophenylhydrazone derivatives; (ii) chromatographic separation of the acidic carbonyl derivatives; (iii) isolation of each component by paper chromatography; (iv) catalytic reduction of the isolated derivatives; and (v) identification of the resulting amino acids by paper chromatography.

The carbonyl derivatives were formed by the method described by Cavallini and Frontali (*3*). The hydrazones were extracted with diethyl ether or ethyl acetate, and the acid carbonyl derivatives were extracted from the solvent with 1*N* Na_2CO_3. The alkaline extracts were washed with chloroform containing 20 percent ethanol, then acidified in the cold with 6*N* HCl. The hydrazones were then reextracted into diethyl ether or ethyl acetate and evaporated to dryness at room temperature.

The derivatives were taken up in a small quantity of methanol and applied in a band about 2 in. from the bottom of a large sheet of Whatman No. 1 filter paper. The papers were developed with butanol, ethanol, and ammonia (7:1:2) (*4*).

The separated bands were cut out and eluted with 1*N* Na_2CO_3, and the eluates were acidified in the cold with 6*N* HCl (*5*). The hydrazones were extracted with diethyl ether or ethyl acetate, and the extracts were evaporated at room temperature. The derivatives were taken up in 1 ml of distilled water and added to 15-ml centrifuge tubes containing about 2 mg of platinum oxide catalyst. Hydrogenation was carried out in a Parr hydrogenation apparatus for

periods varying from 16 hours, as recommended by Meister and Abendschein (*6*), to 5 hours, as recommended by Kun and Garcia-Hernandez (*7*). The clear supernatants were spotted, along with known standards of glutamic acid, alanine, and α-amino butyric acid, on strips of filter paper; developed in phenol and water in an atmosphere of ammonia; dried; and sprayed with ethanolic Ninhydrin.

α-Ketoglutaric and pyruvic acids were present in all the hydrolyzates examined (*8*). The derivatives of these keto acids appeared in three bands, the lowest one running parallel to known α-ketoglutaric acid hydrazone and the higher two running with the two bands of pyruvic acid derivatives (*3*).

In an attempt to explain the presence of α-ketoglutaric acid in the commercial hydrolyzates, the following experiment was made. Five grams of pyruvic acid and 5 g of glutamic acid were added to 150 ml of 10*N* HCl and refluxed for 14 hours, after which time the HCl was distilled off directly (*9*). The remaining semisolid mass was taken up in 100 ml of distilled water, and the solution was filtered. The acid-carbonyls present in the filtrate were isolated and identified by the methods described previously for the isolation and identification of the α-keto acids in the casein hydrolyzates examined.

α-Ketoglutaric acid was present in the filtrate from the acidified and heated pyruvate-glutamic mixture.

Franck and Knoke (*1*) found that during acid hydrolysis of casein the β-hydroxy α-amino acids serine and threonine gave rise to pyruvic and α-ketobutyric acids, respectively. They found, under the conditions of their experiment (6*N* HCl, 14 hours, 140°C), no other α-keto acids.

The logical precursor of the α-ketoglutaric acid found in the commercial casein hydrolyzates would be glutamic acid. The keto acid could arise under the rigorous conditions of hydrolysis (*9*) by the condensation of pyruvic acid initially coming from the degradation of serine, and the glutamic acid would be freed during hydrolysis. This condensation product, presumably a Schiff base, could be rearranged and split in such a manner as to yield α-ketoglutaric acid as one of the cleavage products.

These findings may be of interest in nutritional studies in which acid-hydrolyzed casein provides the source of amino acids in experimental media. If the cultures for which the media are prepared possess active transaminase systems, the fact that α-ketoglutaric and pyruvic acids are present initially in the media might (i) lead to misinterpretation of differences in the levels of amino acids before and after growth (that is, glutamic acid and alanine levels could be affected by transamination involving the α-keto acids present initially in the media); (ii) account in part for the differences in efficiency of casein hydrolyzate media and completely synthetic media in supporting bacterial growth.

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References and Notes

1. B. Franck and J. Knoke, *Chem. Ber.* 90, 2450 (1957).
2. R. E. Neuman and T. A. McCoy, *Proc. Soc. Exptl. Biol. Med.* 98, 303 (1958).
3. D. Cavallini and N. Frontali, *Biochim. et Biophys. Acta* 13, 439 (1954).
4. M. F. S. El Hawary and R. H. S. Thompson, *Biochem. J.* 53, 340 (1953).
5. It was essential to separate each acid carbonyl 2-4-dinitrophenylhydrazone by chromatography before hydrogenation because amino acids from the hydrolyzate were present in the hydrazone mixture; these were eliminated by cutting and elution of the separate hydrazone bands.
6. A. Meister and P. A. Abendschein, *Anal. Chem.* 28, 171 (1956).
7. E. Kun and M. Garcia-Hernandez, *Biochim. et Biophys. Acta* 23, 181 (1957).
8. α-Ketobutyric acid was found in one of the five samples tested. The hydrazone band associated with this keto acid ran slightly above that of the second band of pyruvate in the solvent system employed.
9. J. H. Mueller and E. R. Johnson, *J. Immunol.* 40, 33 (1941).

9 April 1959

Activation of Single Lateral Geniculate Cells by Stimulation of Either Optic Nerve

Abstract. The lateral geniculate nucleus is organized in such a way that, initially at least, information from the one eye is almost exclusively segregated from that from the other eye. Single-unit recording, however, confirms the histological evidence that bilateral integration does take place. A small number of cells (< 8.5 percent) receive afferents directly from both optic nerves and are discharged by stimulating either nerve (direct interaction). More common is delayed interaction, where the cells are discharged independently by either optic nerve but only after a relatively long latency. Indirect interaction effects also occur.

The lateral geniculate nucleus is a synaptic center on the direct path between retina and cerebral cortex. In the higher mammals the acquisition of binocular vision is associated with the development of a partial decussation of optic nerve fibers at the chiasma where fibers from both eyes now pass to each lateral geniculate nucleus. While these changes are taking place, distinct cellular laminae develop in the nucleus, but the fibers from each eye terminate in separate cell layers. Many studies have been made, particularly in the cat, regarding the possibility of binocular integration taking place in the lateral geniculate nucleus. Earlier histological (*1*) and electrophysiological (*2*) studies gave negative results (see *3*). Later, Bishop and Davis (*4*) provided clear evidence of some degree of binocular interaction. At that time this interaction was regarded as being due to extracellular flows of current from active cells affecting the excitability of resting cells in adjacent inactive layers. Recent work in this laboratory (*5–7*) indicates that this factor is probably of minor importance and that the existence in the geniculate of bilateral synaptic connections of varying complexity provides a basis for the small degree of binocular interaction that takes place at this level.

By studying the patterns of degenerating nerve terminals following section of one optic nerve in the cat, Hayhow (*5*) confirmed that each cell layer receives fibers from one eye only. He demonstrated, however, that the interlaminar regions which contain large cells (*nucleus interlaminaris centralis* and *nucleus interlaminaris medialis*) receive fibers from both eyes. This suggests that these regions may be concerned with the integration of information from the two eyes.

The technique of recording from single cells provides confirmation of the supposition that there are cells in the lateral geniculate nucleus which may be activated independently from either eye. Thus, Erulkar and Fillenz (*8*) have recorded from single units which responded to light flashes presented to either eye. Using glass micropipette electrodes filled with 3*M* KCl (direct-current resistance, 5 to 10 megohms) under Horsley-Clarke stereotaxic control, we have now recorded, extracellularly, in the region of the lateral geniculate nucleus, from about 270 postsynaptic units that have responded to electrical stimulation of the optic nerves. Of these, only 23 (8.5 percent) responded to stimulation of either optic nerve with a latency in each case of less than 10 msec. Final confirmation that binocular interaction occurs in the lateral geniculate requires, however, a clear demonstration that the recording sites were actually intrageniculate and that the units concerned were not fibers of passage on their way through the nucleus.

As regards the latter point we now have satisfactory criteria (*9*) which enable us to distinguish between the responses from the region of the cell body (Fig. 1, *A*) and those from an axon (Fig. 1, *B*). In various ways the cell response can be fractionated into the separate components concerned in impulse generation (*9*). Twenty-three units responded to stimulation of either optic nerve with latencies of less than 10 msec.

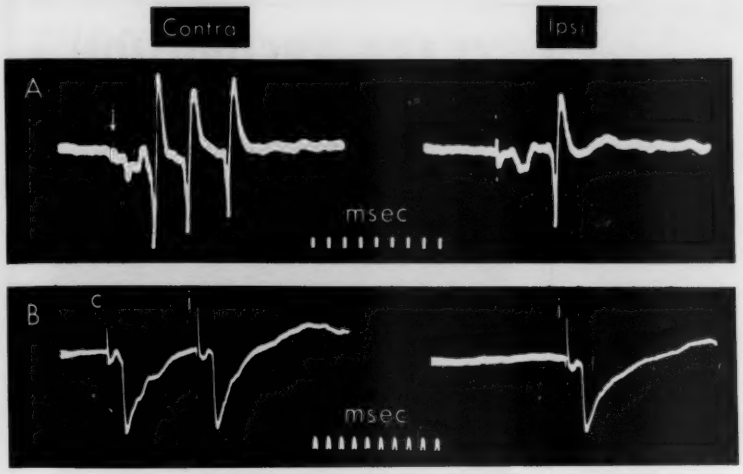

Fig. 1. (*A*) Extracellular responses from the vicinity of the cell body of a single lateral geniculate neuron following electrical stimulation of the contralateral and ipsilateral optic nerves, as indicated. The contralateral response shows repetitive firing. The arrow indicates the position of the stimulus artefact. (*B*) Extracellular responses from single postsynaptic geniculate axons following stimulation of the contralateral (*c*) and ipsilateral (*i*) optic nerves.

Responses from only four were clearly obtained in the vicinity of the cell body and responses from an additional three probably had a similar origin. Responses from the others were derived from postsynaptic axons.

With capillary microelectrodes we have not yet developed a satisfactory method for identifying histologically the site from which the records come, and we have had to rely both on measurements from macroscopic anatomical landmarks and on the general field potentials that result from the massed activity of the geniculate cells. We have, however, recorded identical unit responses with a steel microelectrode and confirmed the geniculate origin of these responses by the iron deposition (Prussian blue) method.

It has been possible to classify our interaction effects into three categories.

1) *Direct interaction.* The most debated aspect of binocular interaction at geniculate level concerns the possibility of direct interaction—that is, whether there are cells in the nucleus that are directly innervated and separately activated by optic nerve fibers from either eye. We consider that most of the 23 units probably fell into this category. However, the rigorous demonstration of direct interaction requires that the response be distinguished as coming from the vicinity of a cell body within the lateral geniculate nucleus (as discussed above) and with latencies to electrical stimulation of the optic nerves brief enough to exclude the possibility that an interneuron intervenes. The latter requirement restricts consideration to the rapidly-conducting group of fibers in the optic nerves. These fibers lead to the discharge of the corresponding geniculate neurons, with a latency of about 1.0 msec (*10*). The great majority of the geniculate cells normally have latencies to optic-nerve stimulation greater than 1.0 msec, so that one would not expect to find many cells in this category.

Of the 23 units only one (Fig. 1, *B*) responded with ipsilateral and contralateral latencies (1.2 msec in each case) less than 1.5 msec. This unit was, however, a postsynaptic axon. Another postsynaptic axon had ipsilateral and contralateral latencies of 1.3 and 1.7, respectively. Many of the units with slightly longer latencies may probably be included in the category of direct interaction if estimates of conduction velocity in presynaptic fibers made from measurements of threshold for stimulation are accepted. At least one of the responses in the latter group was obtained from the vicinity of the cell body. The latency of the geniculate response to photic stimulation of the retina is so long (*8*) that it would be difficult to establish direct interaction by means of light flashes.

2) *Delayed interaction.* The term *delayed interaction* may be used to refer to units which respond to stimulation of either optic nerve but only with a latency long enough to require the intervention of one or more interneurons. No doubt some of the 23 units referred to above fall into this category (short latency, delayed interaction). There are other units, however, which fire only after a latency of 100 to 300 msec (long latency, delayed interaction). We have not looked especially for units of this kind, and any observations we have made have been incidental to other studies. Nevertheless we have found 15 examples out of a total of 122 postsynaptic units recorded in eight experiments.

3) *Indirect interaction.* A geniculate cell may be discharged only by impulses in the one optic nerve, but it is commonly found that impulses in the other optic nerve may influence the firing pattern of that cell (see *8*). As yet we have not studied these indirect interaction effects to any extent.

It is clear that the lateral geniculate nucleus has a more complex function than to serve as a relay station. The direct exchange of information from the two eyes occurs here only to a limited extent, but, in the later stages of the visual process, after the visual messages have reached the cerebral cortex and other centers beyond the lateral geniculate nucleus, bilateral integration at geniculate level becomes much more widespread, possibly involving complex reverberating neuronal circuits. Recent histological studies and single-unit recording in our laboratory have demonstrated the presence of complex corticogeniculate connections.

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W. Burke
R. Davis*

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References and Notes

1. J. L. O'Leary, *J. Comp. Neurol.* **73**, 405 (1940).
2. W. H. Marshall, *J. Neurophysiol.* **12**, 277 (1949).
3. G. L. Walls, *The Lateral Geniculate Nucleus* (Univ. of California Press, Berkeley and Los Angeles, 1953).
4. P. O. Bishop and R. Davis, *Science* **118**, 241 (1953).
5. W. R. Hayhow, *J. Comp. Neurol.* **110**, 1 (1958).
6. P. O. Bishop, W. Burke, R. Davis, W. R. Hayhow, *Trans. Ophthalmol. Soc. Australia*, in press.
7. This research was supported by grants from the National Health and Medical Research Council of Australia, the Ophthalmic Research Institute of Australia, and the Consolidated Medical Research Funds of the University of Sydney.
8. S. D. Erulkar and M. Fillenz, *J. Physiol.* **140**, 6P (1958).
9. W. H. Freygang, Jr., *J. Gen. Physiol.* **41**, 543 (1958); P. O. Bishop, W. Burke, R. Davis, *Nature* **182**, 728 (1958).
10. P. O. Bishop and W. A. Evans, *J. Physiol.* **134**, 538 (1956).

* Fellow of the Ophthalmic Research Institute of Australia.

17 April 1959

Meetings

Forestry

"Forestry and People" is the theme of the 59th meeting of the Society of American Foresters, to be held in San Francisco, Calif., 15–18 November. Headquarters will be the Sheraton-Palace Hotel.

George A. Garratt, president of the society and dean of the Yale University School of Forestry, will open the general session on 16 November. He has announced that all foresters and friends of forestry will be welcome, whether members of the professional society or not.

Nine technical sessions have been scheduled. They will include 60 papers on the subjects of forest management, silviculture, forest products, forestry education, forest recreation, wildlife management, range management, watershed management, and forest economics and policy.

The society expects an attendance of some 1500 members and guests. This meeting holds special interest because of the extraordinary pressures on forest lands created by the rapidly expanding population in the West.

John Callaghan, assistant secretary-manager of the California Forest Protective Association, is general chairman of the meeting, and R. Keith Arnold, director of the Pacific Southwest Forest Experiment Station at Berkeley, Calif., is program chairman. B. H. Payne of the U.S. Forest Service, San Francisco, is the chairman of the arrangements committee. For information write to the society headquarters, Mills Bldg., Washington 6, D.C.

Machine Searching

An International Conference for Standards on a Common Language for Machine Searching and Translation, sponsored by Western Reserve University and the Rand Development Corporation of Cleveland, Ohio, will be held 6–12 September at the Tudor Arms Hotel in Cleveland. International interest has been demonstrated by the submission of 52 papers from ten countries for presentation at the conference. Senator Hubert H. Humphrey of Minnesota will deliver the keynote address at a dinner on 9 September.

Goiter Conference Travel Funds

The fourth International Goiter Conference will be held 5–9 July 1960 in London, England, under the auspices of the London Thyroid Club and the American Goiter Association. The American Goiter Association plans to make available to worthy candidates a limited number of travel grants to enable them to participate in this meeting. Application blanks are available from Dr. John C. McClintock, 149½ Washington Ave., Albany 10, N.Y. Applications will be received until *1 January 1960*.

Forthcoming Events

September

27–30. American Inst. of Chemical Engineers, natl., St. Paul, Minn. (F. J. Van Antwerpen, AICE, 25 W. 45 St., New York 36.)

28–30. American Oil Chemists' Soc., fall, Los Angeles, Calif. (Mrs. L. R. Hawkins, AOCS, 35 E. Wacker Drive, Chicago 1, Ill.)

28–30. Telemetering, natl. symp., San Francisco, Calif. (G. L. Larse, Lockheed Aircraft Corp., Missile Systems Div., Sunnyvale, Calif.)

28–1. Recent Developments in Research Methods and Instrumentation, 9th annual symp. and exhibit, NIH, Bethesda, Md. (J. B. Davis, National Institutes of Health, Public Health Service, Bethesda 14.)

28–2. American College of Surgeons, 45th clinical cong., Atlantic City, N.J. (R. M. Cunningham, Jr., ACS, 40 E. Erie St., Chicago 11, Ill.)

30–1. Industrial Electronics, 8th annual symp., Pittsburgh, Pa. (R. H. Delgado, 954 Brentview Dr., Pittsburgh 36.)

30–1. Mississippi Valley Medical Soc., St. Louis, Mo. (H. Swanberg, 510 Maine St., Quincy, Ill.)

October

1–4. American Soc. of Industrial Designers, Asheville, N.C. (Mrs. R. R. Larisch, ASID, 15 E. 48 St., New York 17.)

1–4. Electrochemical Thermodynamics and Kinetics, annual intern., Vienna, Austria. (M. P. Van Rysselberghe, CITE for the U.S., Dept. of Chemistry and Chemical Engineering, Stanford Univ., Stanford, Calif.)

4–7. American Inst. of Mining, Metallurgical and Petroleum Engineers, fall, Dallas, Tex. (E. O. Kirkendall, AIMMPE, 29 W. 39 St., New York 18.)

4–9. Society of Motion Picture and Television Engineers, semi-annual conv., New York, N.Y. (C. S. Stodter, SMPTE, 55 W. 42 St., New York, 36.)

5–7. Aeronautical Communications, 5th symp., Utica, N.Y. (L. G. Cumming, Inst. of Radio Engineers, 1 E. 79 St., New York 21.)

5–7. Chemical Engineers, annual, Essen, Germany. (Dr. Miessner, VDI-Fachgruppe, Verfahrenstechnik, Rheingaullee 25, Frankfurt-am-Main.

5–7. National Assoc. of Corrosion Engineers, Northeast regional, Baltimore, Md. (T. J. Hull, NACE, 1061 M & M Bldg., Houston, Tex.)

5–8. American Acad. of Pediatrics, Chicago, Ill. (E. H. Christopherson, 1801 Hinman Ave., Evanston, Ill.)

5–9. American Soc. of Anesthesiologists, Bal Harbour, Fla. (J. W. Andes, 188 W. Randolph St., Room 1101, Chicago, Ill.)

5–9. Audio Engineering Soc., 11th annual, New York, N.Y. (AES, P.O. Box 12, Old Chelsea Station, New York 11.)

5–10. Society of Automotive Engineers, aeronautical meeting and aircraft manufacturing forum, Los Angeles, Calif. (R. W. Crory, Meetings Operation Dept., SAE, 485 Lexington Ave., New York 17.)

5–16. Institute of the Aeronautical Sciences, biennial Anglo-American conf., New York, N.Y. (R. R. Dexter, IAS, 2 E. 64 St., New York 21.)

6. American Assoc. of Poison Control Centers, 2nd annual, Chicago, Ill. (A. S. Blank, AAPCC, Connecticut State Dept. of Health, Hartford 15.)

6–8. Aeronautical/Astronautical Problems of High Speed Flight, Stanford, Calif. (E. Haynes, Deputy Director, Aero Sciences Directorate, Air Force Office of Scientific Research, Washington 25.)

6–9. High Temperature Technology, intern. symp., Asilomar, Calif. (Public Relations Office, Stanford Research Inst., Menlo Park, Calif.)

7–8. Advanced Propulsion, 2nd symp. (classified), Boston, Mass. (Lt. Col. P. Atkinson, Propulsion Div., Air Force Office of Scientific Research, Washington 25.)

7–9. Vacuum Technology, symp., Philadelphia, Pa. (American Vacuum Soc., Box 1282, Boston, Mass.)

7–11. International Conv. on Nutrition and Vital Substances, 5th, Konstanz-Zurich, Switzerland. (Secretary General, Benmeroderstrasse 61, Hannover-Kirchrode, Germany.)

8–10. American Assoc. of Textile Chemists and Colorists, natl. conv., Washington, D.C. (G. P. Paine, AATCC, P.O. Box 28, Lowell, Mass.)

8–10. American Ceramic Soc., Bedford, Pa. (F. P. Reid, ACS, 4055 N. High St., Columbus 14, Ohio.)

8–10. American Soc. of Tool Engineers, semi-annual, St. Louis, Mo. (H. E. Conrad, ASTE, 10700 Puritan Ave., Detroit 38, Mich.)

8–10. Biology of Pyelonephritis, intern. symp., Detroit, Mich. (E. L. Quinn, Henry Ford Hospital, W. Grand Blvd. at Hamilton, Detroit 2.)

8–10. Optical Soc. of America, annual, Ottawa, Canada. (S. S. Ballard, Dept. of Physics, Univ. of Florida, Gainesville.)

9–13. American Soc. of Civil Engineers, Los Angeles, Calif. (E. S. Kirkpatrick, ASCE, 33 W. 39 St., New York 18.)

11–16. American Acad. of Ophthalmology and Otolaryngology, Chicago, Ill. (W. L. Benedict, 15 Second St., SW, Rochester, Minn.)

11–16. American Inst. of Electrical Engineers, fall general, Chicago, Ill. (N. S. Hibshman, AIEA, 33 W. 39 St., New York 18.)

11–16. American Soc. for Testing Materials, Pacific area natl., San Francisco, Calif. (R. J. Painter, ASTM, 1916 Race St., Philadelphia 3, Pa.)

12–14. Clay Conf., 8th natl., Norman, Okla. (C. G. Dodd, Eighth Natl. Clay Conf., Univ. of Oklahoma, Norman.)

12–14. Electronics Conf., 15th annual natl., Chicago, Ill. (NEC, 228 N. La Salle St., Chicago 1, Ill.)

(See issue of 21 August for comprehensive list)

APPLICATION FOR HOTEL RESERVATIONS

126th AAAS MEETING

Chicago, 26-31 December 1959

The four hotels for the AAAS Chicago meeting have established special low rates and have reserved large blocks of rooms at each level within the price ranges quoted. Thus everyone making room reservations for the AAAS meeting is assured substantial savings. Further, all confirmations will state the room rate assigned.

The list of hotels and their rates and the reservation coupon below are for your convenience in making your hotel reservation in Chicago. Please send your application, *not* to any hotel directly, but to the AAAS Housing Bureau in Chicago and thereby avoid delay and confusion. The experienced Housing Bureau will make assignments promptly; a confirmation will be sent you in two weeks or less.

If desired, the hotels will add a cot at $3.00 per night—except that all children under 12 are free. Mail your application *now* to secure your first choice of desired accommodations. All requests for reservations must give a definite date and estimated hour of arrival, and also probable date of departure.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

For a list of the headquarters of each participating society and section, see page 228, *Science*, 24 July.

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La Salle	8.00–10.00	10.50–13.00	12.50–15.50	35.50 and up
Sherman	7.45–12.45	11.45–16.45	14.45–19.50	28.50 and up

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ADDRESS .....
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Mail this now to the Housing Bureau. **Rooms will be assigned and confirmed in order of receipt of reservation.**

Letters

Soviet Scientific Publications

The member organizations of the National Federation of Science Abstracting and Indexing Services are very pleased that through the excellent and informative editorial "In perspective" [*Science* **130**, 7 (1959)], the attention of the entire scientific community has been drawn to the improvements in the communication of science information that have been brought about during the past 18 months. However, lest some readers ascribe to Russia more scientific diligence and industry than is justified, I should like to correct one point.

My estimates of the annual abstract and citation coverage by abstracting and indexing services in the United States and Russia referred to the scientific and technical publications *of the entire world*, not the publications of the Soviet Union alone. The total annual Soviet output of such literature probably represents about 10 percent of the world's total; certainly it is no more than 15 percent.

G. MILES CONRAD
Biological Abstracts, *Philadelphia*

Titration Curves

The report "Linear titration curves of acids and bases," by N. R. Joseph [*Science* **129**, 1493 (1959)] calls for comment. A transformation is proposed consisting of the substitution of the operator p for the operator "–log" in the standard Henderson-Hasselbalch equation. The resulting straight-line plot, $pA–pB$ against pH, does not differ except in labeling of the axes from a plot of log B/A against pH. This plot has undoubtedly been used often to show roughly the goodness of fit of an experimental set of points on a titration curve of a monovalent acid, or of polyvalent acids with widely separated proton donor groups. The transformation per se does not avoid the use of a logarithm table, for how else can one obtain the p values? If semilogarithmic graph paper is used, it is the logarithmic scale which makes a logarithm table unnecessary, not the "transformation." When such a graph is used, division is needed to obtain A/B instead of the subtraction of the two logarithms that is necessary when regular grids are used.

In the particular illustrative example used, four reactions involving H^+ are stated and "four transformed mass action law equations" are given. It is apparent that $A_2 = B_1$, $A_3 = B_2$, and $A_4 = B_3$. The author's statement that his Fig. 1 "clearly indicates the distribution of electrical charge over the molecule as a function of pH" is misleading, because of the failure to make these identifications (*1*). The figure indicates that the four mass action law equations are independently solvable when in fact they are simultaneous equations and only if the pK's are far apart is it possible to make the necessary approximations to solve them independently. The lines given end arbitrarily at $pA–pB = \pm 2$ and give a discontinuous appearance to what is in fact a smooth continuous titration with only a slight "break" between the third and fourth group as given in the original paper (*2*).

A very important feature of traditional titration curves is lost in Joseph's transformation." It is difficult or impossible to "add" the segments of titration for the four pK's in the transformation, whereas this is a simple matter with traditional plots. Thus, it is awkward to obtain a valid comparison between experimental and constructed curves in the form used by Joseph for any polyvalent acid when the groups are not widely separated.

The usefulness of the d'Ocagne nomogram proposed is not apparent. Most people would find it easier and more accurate to do the simple subtraction necessary to determine the difference between pH and pK rather than to use the nomogram. The same is true for the other possible combinations for which the nomogram might be used. D'Ocagne nomograms are useful when relationships are complex, but not when the arithmetical relations are as simple as the one demonstrated.

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References and Notes

1. J. T. Edsall and J. Wyman, Jr., [*Biophysical Chemistry* (Academic Press, New York, 1958), vol. 1, chap. 9] point out that for each charge type there are a number of "microscopically different species" differing in the location of the charge(s) but not in net charge. Thus, in the example used by Joseph there are four microspecies included in $A_2(B_1)$, six in $A_3(B_2)$, and four in $A_4(B_3)$. This important aspect of distribution of charges on a molecule is neglected here, as it was by Joseph.
2. J. P. Greenstein and N. R. Joseph, *J. Biol. Chem.* 110, 619 (1935).

In his letter Levy has criticized some of the procedures and results described in my recent report. The questions raised are of two kinds, mathematical and chemical, and will be discussed in that order.

A glance at the earlier report [*Science* **128**, 1207 (1958)] would have shown Levy that the symbols pA and pB were used to explain the construction and op-

eration of a semilogarithmic pH calculator. This yields the term $(pA - pB)$ in place of the usual log B/A; in all subsequent equations or graphs the terms may be used interchangeably. The usage was retained in the second report in formulating two nomograms for glycyl aminotricarballylic acid. If the pH range of the straight lines of Fig. 1 be extended beyond four pH units, the apparent discontinuities noted by Levy do not appear. When, as in the d'Ocagne nomogram (Fig. 2), the pH scale runs from 2 to 10, no breaks occur.

The physicochemical questions concern the number of ionic species required for a given polyelectrolyte. Since each titrable group involves two forms, a proton-donor A and an acceptor B, the number of *mathematically* possible species s is 2^n, where n is the number of titrable groups and pK's. For glycyl aminotricarballylic acid, n is 4 and s is 16. When n is 10, s is 1024; when n is 20, s exceeds 10^6. Proteins may contain 100 or more titrable groups; when n is 100, s is about 10^{30}. This is more than 10^6 moles; if a molecular weight of 10^5 is assumed, the weight is about 10^8 kilograms. Obviously, only an infinitesimal fraction of the mathematically possible species can or should be considered.

The number of equations necessary to represent a complex polyelectrolyte is much nearer to n than to s. For glycine, n is 2 and s is 4. One of these is the uncharged neutral molecule present to the extent of about one molecule in 10^8. The curve requires two equations, relating three ionic species. When n is 4, s is 16. If these are tabulated for glycyl aminotricarballylic acid, 8 or 10 of the 16 forms are found to be of very low probable occurrence, as for example the uncharged neutral molecule. The curve may be described by four pK values relating five ionic species. For higher values of n, s becomes successively 32, 64, 128, and so on. For most purposes the distribution of charge is given by n equations and pK values, relating $(n+1)$ ionic species. A generalized nomogram is derived on this basis.

Levy, apparently well content with algebraic formulations, considers nomograms superfluous. Others, seeking elegance, find nomograms useful and rewarding. In a system containing several polyelectrolytes, algebraic formulations and curvilinear diagrams become inadequate. In biological systems there are large numbers of simultaneous reactions involving not only hydrogen ions but also other cations and anions. There are also numerous oxidation-reduction reactions which depend on pH. Algebraic formulations consist of numerous simultaneous equations. When the number exceeds five or ten, it is difficult for one not using visual aids to coordinate all the simultaneous processes. Geometrical transformation to curved polydimensional surfaces is difficult and does little to clarify the relations. Formulation of the equations as straight lines and construction of nomograms go far toward simplifying these problems. This is a well-established procedure in many branches of science. At certain levels of complexity two-dimensional linear nomograms become preferable not only to alegbraic formulations but also to curved polydimensional surfaces or their projections.

I find no statement in either of my reports asserting nonexistent advantages over standard methods. In the second, the entire emphasis was placed on the construction of a simple linear d'Ocagne nomogram illustrative of general methods for complex problems. By these methods diagrams based on three rectangular coordinates are easily transformed to nomograms with three or more parallel coordinates.

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New Products

The information reported here is obtained from manufacturers and from other sources considered to be reliable, and it reflects the claims of the manufacturer or other source. Neither Science *nor the writer assumes responsibility for the accuracy of the information. A coupon for use in making inquiries concerning the items listed appears on page 518.*

■ TRIAXIAL ACCELEROMETER has three piezoelectric sensing elements in mutually perpendicular planes within a ⅝-in.^{3} block weighing 1.4 oz. First resonance frequency is 30 kcy/sec. Sensitivity is 5 mv/*g*. Dynamic range is 1000 *g*, and transverse sensitivity is less than 5 percent. Temperature range is −65° to +220°F with maximum sensitivity change ±10 percent. Frequency response is 2000 cy/sec ±5 percent with a 1000 megohm load. (Endevco, Dept. 9)

■ BAND-ELIMINATION FILTER is designed to reject any frequency between 20 and 20,000 cy/sec and to pass other frequencies between d-c and 100 kcy/sec. A direct-reading dial tunes over the full range. Input and output impedances are 600 ohm. Dynamic range is more than 120 db, and the rejected frequency is reduced by more than 40 db over most of the range. The filter is a passive circuit requiring no power supply. (Allison Labs., Dept. 12)

■ COLORIMETER ACCESSORY is a sample spinner that makes it possible to measure reflectance of samples of fabrics with directional weaves and of threads, yarns, and synthetic fibers wound on cards. The sample holder is a manually spun flywheel with concentric retaining rings for mounting the sample. (Instrument Development Laboratories, Dept. 16)

■ COMPUTER-MODULE TESTER performs automatically static and dynamic tests on a "go" or "no go" basis. Up to nine tests or test programs, including speed, response and continuity, may be performed on each module circuit. The equipment is said to be capable of testing switching response as fast as 10 mμsec and as slow as 500 mμsec with precision ±3 percent or ±3 mμsec, whichever is greater. (Atronic Products Inc., Dept. 17)

■ NEUTRON SOURCE CONTAINER is an aluminum vessel 35 in. long and 23 in. in diameter filled with 350 lb of paraffin. For nuclear-physics teaching, a neutron source can be positioned in the container at the level of its two cadmium lined ports, to produce a collimated beam of neutrons. The equipment includes source-handling tongs, two detector mounts, and a variety of foils, foil holders and Lucite spacers that fit the two cadmium-lined port drawers. (Nuclear Chicago, Dept. 20)

■ OSCILLOSCOPE permits observation of very short pulses that are repetitive. The instrument uses a stroboscopic sampling technique whereby successive pulses are sampled at different points. The samples taken over a period of about 1 msec are amplified and displayed on the screen of an ordinary low-frequency cathode-ray tube. Sampling time is said to be 0.0004 μsec, sensitivity greater than 5 mm/mv. Pulses can occur at random as well as periodically (Lumatron Electronics, Inc., Dept. 22)

■ TRANSISTOR-AMPLIFIER RELAY is said to require less than 10 μw for positive relay actuation. Standard contact arrangements are single pole, double throw or double pole, double throw for contact loads up to 3 amp at 115 v a-c or 29 v d-c, noninductive. Various contact materials are available. Bias voltage requirement is 16 to 25 v d-c. Sensing currents 10 times normal are tolerated. Temperature range is 32° to 122°F. (General Automatic Corp., Dept. 26)

■ TEMPERATURE MEASUREMENT SYSTEMS cover the range from −320° to +2000°F with resistance-temperature transducers. The systems are of modular design with bridges and power supply. Resistance variations are changed into voltage variations with 5-v full-scale output for direct reading or input to recorder. System accuracy up to ±0.1°F is available. (Cardinal Instrumentation Corp., Dept. 35)

■ PHOTODIODE is a diffused *n-p-n* silicon diode that allows current to flow when either junction is illuminated. Any biasing voltage up to 50 v will operate the device. Response is up to 1200 μa for 1200 ft-cas. Current in the dark is less than 0.5 μa. The device is 0.5 in. long and 0.085-in. in diameter. A minute glass lens is located at the end opposite the leads. Temperature range is −55° to 125°C. (Texas Instruments, Dept. 11)

■ FUNCTION GENERATOR accepts input in the form of a double pencil-drawn curve on ordinary graph paper and produces electrical output signals proportional to the ordinate as a function of time representing the abscissa. The graph is carried on a drum that can be adjusted to rotate once in 10 to 300 sec. A servo-driven stylus follows a null position midway between the two pencil lines that define the function. Parts of the curve may be erased and changed at will. Controls include a hold button that stops drum rotation and a reset knob that permits advancing or reversing the drum during a hold period. (Research, Inc., Dept. 19)

JOSHUA STERN
National Bureau of Standards, Washington, D.C.

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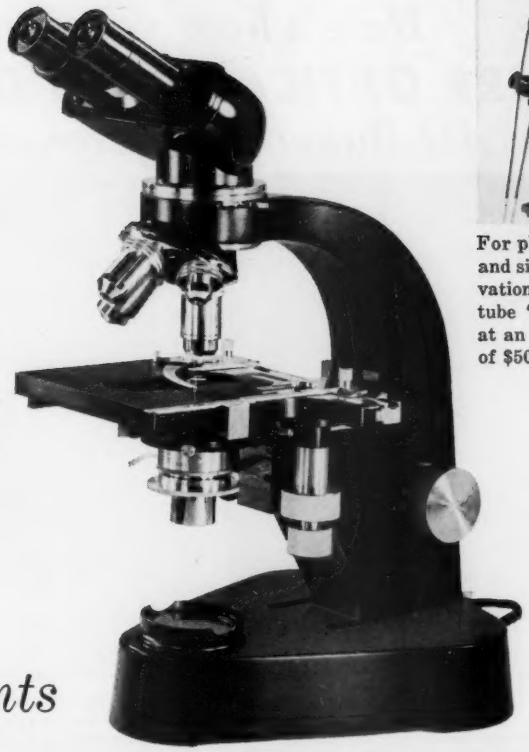

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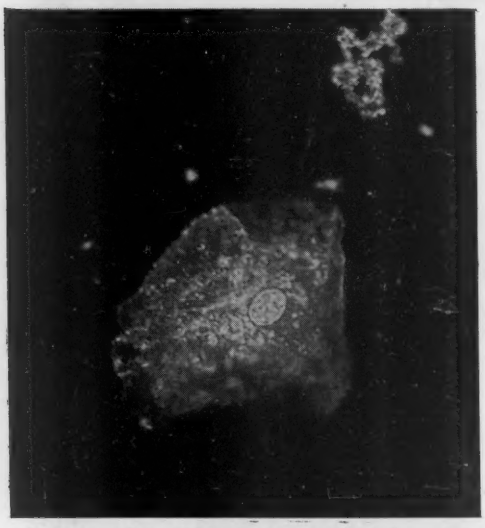
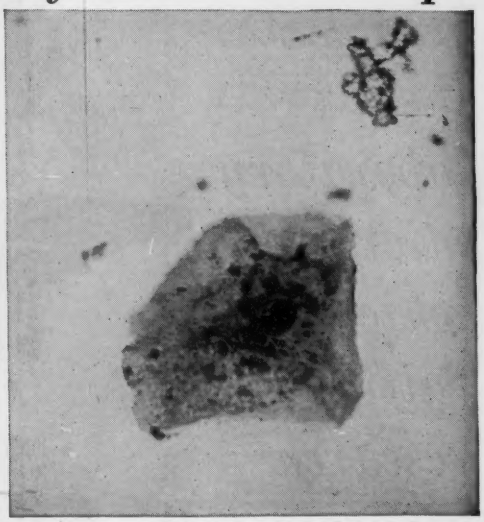


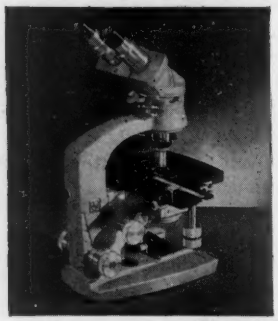
1. First, as shown in the photomicrograph* above, the microscope analyzer was rotated until the background was brought to extinction. Readings were taken directly from the analyzer scale. Averaged settings resulted in reading of 70.4°.



2. Next, the analyzer was rotated until the nucleus of the cell was brought to extinction. Averaged settings resulted in reading of 138.2°.

3. The Optical Path Difference, in degrees, is *twice* the difference between the two readings:

$$OPD = 2\,(138.2° - 70.4°) = 135.6°, \text{ or } OPD = \left(\frac{135.6°}{360°}\right).546 = .206 \text{ Microns.}$$



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While the AO-Baker Interference Microscope is primarily a quantitative instrument, it also offers unique advantages for qualitative observations through variable intensity contrast and dramatically effective variable color contrast.

*Photomicrographs taken by Mr. Lynn C. Wall, Medical Division, Eastman Kodak Co. Data: Epithelial Cell. AO-Baker Interference Microscope, 40X Shearing objective, 10X eyepieces. Corning filter CS4-120 with AO Model 630 Pulsarc Illuminator to transmit monochromatic light at .546 microns.

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